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SOCIETY OF ENGINEERS

ESTABLISHED MAY 1854

Journal and

TRANSACTIONS FOR 1908

EDITED BY

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A.M. Inst. C.E., M.R.S.I.
SECRETARY.

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SOCIETY OF ENGINEERS.

ESTABLISHED MAY 1854.

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OFFICES:

17 VICTORIA STREET, WESTMINSTER, S.W.

Hours, 10 *a.m. to* 4 *p.m.*; *Saturdays*, 10 *a.m. to* 1 *p.m.*

TELEPHONE: 244, VICTORIA.

PLACE OF MEETING.

THE ROYAL UNITED SERVICE INSTITUTION, WHITEHALL.

4TH MARCH, 1908.

PAPERS AND PREMIUMS.

The Council of the Society of Engineers invite original communications from Members and Associates, as well as from gentlemen who do not belong to the Society, on subjects connected with any branch of Engineering.

For any papers that may be considered sufficiently meritorious the Council may at discretion award one or other of the following Premiums, viz. :—

1. The President's Premium, given annually by the President, and consisting of a Gold Medal of the value of Five Guineas.

2. The Bessemer Premium, provided for annually by the late Sir Henry Bessemer, F.R.S., Honorary Member, of the value of Five Guineas.

3. The Society's Premiums, given annually by the Society, of an aggregate value not exceeding Twenty Pounds.

The number and value of the Society's Premiums are decided by the Council according to the number of meritorious papers read during the year.

By the Rules of the Society, Members of Council are disqualified from receiving Premiums for Papers.

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SOCIETY OF ENGINEERS'
TRANSACTIONS FOR 1908.

February 3rd, 1908.

INDUCTION OF THE PRESIDENT,
JOSEPH WILLIAM WILSON.

Mr. Maurice Wilson, President for the year 1906, occupied the Chair at the commencement of the meeting.

The Minutes of the Ordinary Meeting held on the 2nd December, 1907, were read, confirmed, and signed.

The names of seventeen Candidates, proposed for election, were read by the Secretary.

The Chairman: In the absence of the President, I now have pleasure in presenting the premiums awarded during the past session.

The Premiums were then distributed.

Mr. R. W. A. Brewer, in acknowledgment of the President's Gold Medal awarded to him, said: Mr. Chairman, it is with much pride and pleasure that I accept this award at your hands, and I trust that you will convey my sincere thanks to the President and to the members of the Council. I thank you very much for your kindness in selecting my paper, and I hope that at some future date I shall have the pleasure of reading another paper before the Society of Engineers.

The Chairman: I have to express the thanks of the Society to the authors of the other papers read during the past session: Mr. B. H. Thwaite and Mr. R. F. Thorp for their joint paper on "The Renard and Sourcouf Road-Train System"; Mr. E. R. Matthews for his paper on "Waterworks Construction in America"; and Mr. H. C. Huggins for his paper on "Bridle Roads in the West Indies." I take it, gentlemen, that you would like to express your thanks. (Applause.)

I have no doubt that practically everybody present is aware of the fact that our President for 1907 has been absent in China, as far as I can remember, for almost the whole year. I believe he is still there. At any rate, which is the most important matter—he is not present to occupy the Chair this evening, and I take it that probably I am the only person present who does not regret his absence, because it gives me the opportunity of

personally taking part in what I feel is really a unique occasion —in this way. I am sure that you will forgive me for expressing a certain amount of, perhaps, pardonable pride in the fact that, as far as I can ascertain, this is the first occasion on which two brothers have ever occupied the presidential chair in any engineering Society or institution in the world. It is a "record" also that a president of our Society should have worked his way right through from a member of Council to the presidency, and then started all over again, and should have gone through the whole process a second time. It is a process which those who have never been on the Council cannot realise. Our new president has done this in a way that, I am sure, will commend itself particularly to the members of the Society. And, lastly, it gives me an opportunity of performing a task which, perhaps, is slightly Gilbertian—that of a member of a family decorating his eldest brother. With these few remarks, gentlemen—I will not trespass further upon your time—I have great pleasure in installing my eldest brother into this honourable position, in handing to him his certificate of presidentship, and in hanging round his neck this badge which has been hidden from your sight for so long, and which, I am sure, it will gladden your hearts to see reposing on the bosom of one who so very worthily deserves this honour. ("Bravo." Loud cheers.)

The Chairman then invested the incoming President, Mr. J. W. Wilson, with the presidential badge, and added: I wish you every success during your year of office, and I have no doubt that you will occupy the position in the way that you did many years ago, or if possible, with even greater éclat than in the year 1892.

Mr. George A. Goodwin: I also have the pleasing duty to refer to our President, but not in the way that his brother has done, and not in the way that another member of the Council will do later on. I think that you will all agree with me that we owe Mr. Wilson as an individual a very great debt of gratitude indeed for his services during the past year. As you know, Mr. St. George Moore—fortunately for him, but unfortunately for Mr. Wilson in particular—has been called to China, where, I believe, he is now, and he was only able, as far as I remember, to attend one meeting during his year of office, and that was for his own Presidential address. I am sure that you will agree with me that Mr. Wilson acted with great kindness in coming forward as Acting President during the last twelve months. His great experience, gained when he was President some sixteen years ago, and over eighteen years of constant service as a member of the Council, has enabled him

to give us the benefit of his very valuable help in steering the ship of the Society of Engineers into a haven of rest where he now takes control. I think most of us know he is engaged all the year round at the Crystal Palace School of Engineering, where he has many duties to fulfil, and we must appreciate all the more his kindness and the management of his time which has enabled him to attend all the meetings of this Society, and give us his services. I do not think that he has missed one meeting. Therefore, I will ask you with all kindness to give him a hearty vote of thanks for his services during the past twelve months. (Cheers.)

Mr. Percy Griffith: Gentlemen, as a matter of form, this resolution requires seconding, not, however, from your point of view, because I am sure that your enthusiasm is sufficient to warrant you receiving it at Mr. Goodwin's hands without any seconding whatever, both on account of its proposer and on account of its object. There is one fact which Mr. Goodwin did not mention, perhaps, as emphatically as he might—namely, that not only has our excellent President had thrown upon him the presidential duties for two years in succession, but during this past year those duties have been more than usually onerous, and the responsibilities have been more than usually great. To anyone familiar with the working of institutions of this sort, it will be realised that no more trying event can happen to a president than to lose the secretary during his year of office. Now, that happened in this case, and it has involved our Acting-President in responsibilities and labours of which no one unfamiliar with the same circumstances can have any idea. Our great delight, as members of the Council, has been that under this special strain and difficulty we have not had any doubt or fear for the future; and you, as members, I am quite sure, have had no occasion to notice any irregularity or failure of any sort. That has been due to the fact that we have had at the head of affairs a past President now, happily elected to the position of President for a second time, and one who, because of his past experience as well as because of his innate ability, has been capable of meeting every emergency that has arisen in the most admirable and perfect way. From a member of the Council, you will, I am sure, accept this as meaning more than can be known to you as members. In every detail we have been perfectly safe as well as perfectly content under the Chairmanship of our now elected President. I therefore have much pleasure in seconding the vote of thanks to the Acting-President for his services during the past year.

The vote was carried by acclamation.

The President: Mr. Goodwin, Mr. Griffith, and gentlemen,

I thank you for the very kind manner in which this vote has been proposed, and for the cordial way in which it has been received. It is the privilege of a senior Vice-President to act as under-study to the President. It does not often involve doing most of his work for him, it is true; but, at the same time, it is the fortune of war, and I should be very sorry for this occasion to go by with an idea that I did nearly all of the work. I did the best I could, but it would be wrong for me not to tell you how much the members of the Council have done in supporting me during this trying year; and more particularly, that our worthy Honorary Treasurer, Mr. Butler, has also had put upon his shoulders many extra duties which, I am sure, he did not anticipate at the beginning of the year. I thank you, gentlemen, and I appreciate very much the friendly way in which you have responded to this proposal.

The President then delivered the following Inaugural Address:—

Gentlemen,—Sixteen years have elapsed since I was elected President of the Society of Engineers, and my first duty this evening must be to thank you for having again conferred this honour upon me. Having joined the Society thirty-six years ago, and having served continuously upon the Council for twenty-one years, I may claim to know something of its welfare and progress; and it will be my endeavour to promote these to the utmost of my ability during my year of office, knowing how thoroughly I may depend upon the cordial co-operation, not only of the Council, but of all the Members of the Society; so that together we may be able to raise it to a still higher position than it already occupies.

The details of our progress during the past year will be found in our Annual Report; so that I need only refer to one or two events which have taken place during that time, as more especially affecting our welfare.

The Society has suffered heavy personal losses during 1907. The year began with a blank in the List of our Honorary Members, caused by the death of Sir E. J. Reed. This we were pleased to be able to fill by the election of Sir Edward Raban, Director of Works to the Admiralty. Shortly after this, the sudden death of Sir Benjamin Baker, Past President of the Institution of Civil Engineers, not only deprived the world of one of her greatest engineers, but also removed one of our highly valued Honorary Members and Trustees. It has afforded us all great gratification that Sir William Matthews, President of the Institution of Civil Engineers this year, has consented to fill this gap in our List of Honorary Members; and that Sir Alexander

W. B. Kennedy, Past President of the Institution of Civil Engineers, and already an Honorary Member, has agreed to act as one of our Trustees, with Sir Douglas Fox and Sir William H. White.

In June last our old friend and colleague, Mr. Perry F. Nursey, passed away at the age of seventy-seven. Joining the Society fifty years ago, he always took a keen interest in its welfare, to which indeed he largely contributed. He acted successively as Secretary, Member of Council, and President in 1896. He then succeeded the late Alfred Williams as Honorary Secretary and Treasurer; and during the last few years he again occupied the position of Secretary. Of his untiring devotion to his duties, and of the ability with which he discharged them, as well as of the personal respect entertained towards him by all Members of the Society, especially by the Members of the various Councils with whom he co-operated, it is needless for me to speak. Last October, addressing you from this chair, I endeavoured to lay them more adequately before you. I will only add that, by his death, we have lost one of the mainstays of the Society: and I cannot do better than record here the Resolution which was unanimously agreed to on the above-mentioned occasion:—

"The President, Council, and Members of the Society of Engineers, at this their first Meeting after the death of their Secretary, Mr. Perry F. Nursey, desire to place on record their appreciation of the invaluable services rendered by him to the Society during his long and honourable life; and to express to his widow and his family their deep sympathy with them in their bereavement."

Since then the Members of the Society and their friends have subscribed a sum of money, the interest of which will provide, from time to time, a "Nursey Premium"; so that the name of our good old friend and fellow-worker will be worthily perpetuated amongst us.

It may here be recorded that the interest upon a legacy left by our Past President, the late Mr. Joseph Bernays, will also provide, from time to time, a "Bernays Premium."

The Council have appointed Mr. A. S. E. Ackermann Secretary, in succession to Mr. Perry F. Nursey, and we all feel confident that the Society will continue to flourish under his able management.

In July last we lost our Vice-President, Mr. William H. Holttum, who had rendered valuable service to the Society since he joined the Council in 1900. We were all looking forward with pleasurable anticipation to his Presidency in due course next year.

Finally, on December 23rd, it fell to my lot to represent the Society at the funeral, in Westminster Abbey, of the late Lord Kelvin, who had been one of our Honorary Members since 1890. By his death the world has been deprived of one of its greatest scientists; who, by the noble utilisation of his transcendent genius, has probably done more than any man since Newton to investigate and turn to practical account the hidden mysteries of science; and thus to further develop them "for the use and convenience of man." He was the first man of science in this country to be raised to the Peerage, and many were the honours showered upon him. Yet it is, surely, the greatest honour of all that his profound scientific knowledge did but confirm his belief in God, and lead him to the conclusion that science, instead of being opposed to religion, was indeed one of its greatest supporters. The walls of the Meeting Room of the Institution of Civil Engineers bear the names of the world's leading engigineers and scientists. As "Newton" is among them, let us hope that "Kelvin" also may be added in due course.

In reference to the past I need only add that we have had, on the whole, a successful year in 1907. The Medal and Premiums which have been presented this evening have been awarded for valuable papers which have evoked excellent discussions. The professional visits which we paid to the "Reservoirs of the Metropolitan Water Board" at Honor Oak in June, and to "Chatham Dockyard" in September, were in all respects satisfactory, though the Members present at them, as well as at our Ordinary Meetings, have been hardly up to the average number.

Our Financial Position continues to be eminently satisfactory.

The interval that has elapsed since my former Presidential Address, though but half a generation, yet includes several notable examples of Engineering progress; and to one or two of these I will briefly refer.

The only Tube Railway then in operation was the partially completed "City and South London" electric line.

It is not necessary for me to recapitulate the names of its many worthy imitators. Like all pioneer undertakings it had to bear the brunt of much adverse and pessimistic criticism; but it has proved its value; and great is the development which has followed in this direction. Yet the miles of Tube Railways and Electric Tramways, with the Motor Buses which run in connection or in competition with them, carrying as they do many millions of passengers yearly, hardly appear to have done much to relieve the traffic congestion of our great city.

Much indeed yet remains to be accomplished, and many excellent schemes have been brought forward by those well qualified to deal with the problem.

The paramount importance of the recent Royal Commission on "London Traffic," and its monumental report, are known to all. An important paper on the subject was read to us here in 1905 by Messrs. C. S. Meik and W. Beer; and Mr. W. N. Twelvetrees devoted his late Presidential Address to the Civil and Mechanical Engineers Society to another aspect of the matter. It is evident that the question is one of such magnitude and complexity, that only exceptional powers will suffice to grapple with it successfully.

In my last address I detailed the proposals brought forward by Sir Christopher Wren after the great Fire of London; which, if adopted, would have ensured at any rate a more systematic and convenient basis for the development of London. His plans were not adopted; and to-day London is a congeries of cities, huddled unsystematically together, and spreading rapidly farther and farther afield, with no supreme authority to direct or control the general development. Yet if the present rate of increase be maintained, the population of the Metropolis will have more than doubled in less than half a century, being prophetically estimated at 16,000,000 in 1960; and it is appalling to consider what the Traffic Problem will then be, if the present erratic and unmethodical growth be permitted to go on unchecked. There are several minor improvements which could be carried out without difficulty or delay. Slow, heavy, traffic should be diverted from the main thoroughfares, at any rate during the busiest hours of the day; direction facilities should be increased by displaying the names of streets on all lamps; maps of the surrounding district should be accessible at all railway stations, and on them the numbering of the longer streets should be indicated. Some control should also be exercised over the routes taken by the various omnibuses, which at present tend in some instances to unreasonably disturb quiet residential districts, or to interfere with occupations requiring an absence of vibratory unrest. In my former address I drew attention to this matter, and urged that "before it is too late some well defined scheme for the gradual reorganisation of existing London, and its duly systematic development outwards, especially with regard to the means of Traffic, should be decided upon by a Congress of Engineers and other Authorities most interested in the matter." I can but reiterate this, and trust that before long a definite move may be made in a matter which is of national importance.

The present position and future development of the Port of London is another question of paramount importance, and of rapidly increasing urgency. It is undeniable that much yet remains to be done to bring up to date and develop yet further the existing dock accommodation, and many proposals have recently been brought forward for this purpose. The latest Board of Trade returns indicate that there is now little difference between the total trade of London and Liverpool; the two ports together accounting for considerably more than half the over-sea trade of the United Kingdom.

Yet, while at Liverpool, Manchester, Southampton, Avonmouth, Cardiff, Dover, Barry, and other places, many millions have been and are being expended upon the extension and improvement of harbour and dock accommodation, little or nothing is being done for the Port of London.

The Manchester Ship Canal, which was under construction when I last addressed you, is steadily justifying its existence; and it is significant that the Port of Manchester is continuously developing its trade, which has increased by some 50 per cent. during the last few years. Owing, however, to the uncertainty of the London Port problem, and the many conflicting interests connected therewith, matters are at present in a state of abeyance; and it is to be hoped that it may not be long before a comprehensive and statesmanlike scheme may be put in hand, for only thus can the decline of London's pre-eminence in this respect be averted.

One of the schemes proposed involves the diversion of a portion of the Thames. This has been looked upon as a new idea, but that it is not so a reference to Pepys' Diary will prove.

In a letter written to him in 1699 the writer says: "I had preached for Mr. Gataker at Redriff [Rotherhithe] and next morning I walked with him over the fields to Lambeth. He showed me, in the passage, diverse remains of the old channel, which had heretofore been made from Redriff to Lambeth, for diverting the Thames whilst London Bridge was building, all in a straight line, or near it; but with great intervals which had been long since filled up for it is more than 50 years ago." There appears to be no other record of this important diversion of our river; but an inspection of the map of London will show that the course indicated would be through very low-lying districts, while some of the existing names of streets, such as "New Cut," "Lower Marsh," etc., are certainly significant.

Sixteen years ago we saw the projection of the great Trans-Siberian Railway, which has since been carried out; and which,

whether viewed from a strategic, commercial, or economical point of view, can hardly be said to have realised the expectations of its promoters.

To-day we see in progress another Trans-Continental Railway; which, if it does not circle quite so far round the globe, yet surpasses in magnitude and importance, as a whole, any previously conceived scheme of railway construction; which is of the greatest importance to us from an Imperial point of view; and which, it may be confidently anticipated, will fully realise the great expectations that are foretold for its near future.

The "Grand Trunk Pacific Railway of Canada" is the natural outcome of that great system of nearly 5000 miles of existing lines known as the Grand Trunk Railway, of which Sir C. Rivers Wilson, G.C.M.G., is the President. Mr. Charles M. Hays, who occupies the position of Second Vice-President and General Manager of the Grand Trunk Railway, is the President of the new railway; and Mr. Frank W. Morse, formerly Superintendent of Motive Power, and for the last few years one of the Vice-Presidents of the Grand Trunk Railway, is Vice-President and General Manager of the new railway; while the Chief Engineer is Mr. B. B. Kelliher, who has great experience in the construction of trans-continental railways in the United States; having formerly also been engaged upon railway construction in his native country of Ireland.

The Grand Trunk Pacific Railway, which will be the only "all Canadian route" from her eastern to her western shores, was incorporated in 1903, and is being vigorously pushed forwards towards completion, which is due by the end of 1911.

The Main Trunk Line will have a length of 3600 miles; and the eastern half, from Monckton on the Atlantic to Winnipeg, the great central Canadian city, is being constructed by the Canadian Government, who will lease it to the Grand Trunk Pacific Railway Company for 50 years, with the option of renewal. During the first 7 years there is to be no rental; during the next 3 years a rental of 3 per cent. will be paid, provided the net earnings prove sufficient; and a rental of 3 per cent. upon the cost of construction for the rest of the period.

The western half, from Winnipeg to Prince Rupert, the terminus on the Pacific coast, is being constructed by the Grand Trunk Pacific Railway Company; the Canadian Government guaranteeing 2600*l.* per mile on the "Prairie Section," and three-quarters of the cost on the "Mountain Section."

The estimated cost of the Eastern Division, 1800 miles long, is 6000*l.* per mile; of the first 1100 miles of the Western Prairie Section 5000*l.* per mile; and of the remaining 700 miles, or the Mountain Section, 10,000*l.* to 12,000 per mile; giving a

total approximate cost for the main line alone of 25,000,000*l.*; while the rolling stock will absorb 5,000,000*l.*

In the earlier railways constructed on the American continent frequent use was made of steep gradients and sharp curves. This, though mitigating the cost of initial construction, proved to be a mistaken policy; as the necessary subsequent modifications were often so costly as to approximate the amount spent upon the original construction.

In the Grand Trunk Pacific Railway no such mistake will be made; the engineers who are locating the line being limited to a grade of 31 feet to the mile in the Eastern Section; and of only 26 feet to the mile in the Mountain Section. As a contrast to which it may be mentioned that the Canadian Pacific Railway in crossing the Rocky Mountains has a maximum grade of 237 feet to the mile.

Thus, although the initial cost may be somewhat increased, the subsequent outlay will be materially reduced; while from the outset the economical running of the traffic cannot fail to be assured.

The latest official report shows that as regards the Eastern Division all preparations for the construction of the railway are well in hand, and that detached portions are already under construction. This line is to cross the St. Lawrence River 5 miles above Quebec by the great bridge now under construction by the "Quebec Bridge and Railway Company." This is to be the largest cantilever bridge in the world, having a centre span of 1800 feet, or 90 feet more than that of the Forth Bridge, with a total length of 3240 feet and an elevation of rail above high water of 150 feet; thus affording a free passage for all ocean steamers.

At Winnipeg, where the Eastern and Western Divisions meet, will be erected a Great Union Station, designed to accommodate the Trans-continental, the Grand Trunk Pacific and the Canadian Northern Railways.

As regards the Western Division, the Prairie Section of the main line, from Winnipeg to Edmonton, 793 miles long, is all under contract; 701 miles have been graded, and 320 miles of track are already laid, while the contract for the remaining 118 miles beyond Edmonton has just been let, and the whole of the Prairie Section is expected to be open to traffic before the close of the present year.

The Mountain Section is naturally not so far advanced; but the survey parties are approaching the completion of location, and contracts for this, the most arduous portion of construction, will be let before long.

Of the various branch lines, many of which will be com-

menced before the main line is completed, the most important will be that from "Fort William" to "Lake Superior Junction," 200 miles long; of which 170 miles have been graded, and 96 miles laid. This branch will give access to the navigation of the Great Lakes, by means of which direct water communication will be opened up for the newly developed agricultural and other wealth of the country with the rest of the world.

Powers have also been secured for lines of railway to Fort Churchill on the western shore of Hudson Bay, and to Dawson in the Yukon District of the far North-West; connections will also be made with the existing Grand Trunk Railway, the pioneer railway of Canada.

The carrying out of such an undertaking cannot be achieved without a display of more than ordinary courage and perseverance on the part of those engaged in its location and construction. In order to determine the best route for the railway across the great range of the Rocky Mountains, a large staff of engineers has been in the field during the last three years, making exhaustive investigations of the various passes; that which is known as the "Yellowhead Pass" having been ultimately adopted.

By this route the passage of the Rockies will be of a much more favourable nature than on any other existing railway, there being but one summit of 3712 feet altitude to cross; whereas the Canadian Pacific Railway has two summits, one of 5299 feet and one of 4308 feet.

The site for the terminus of this great line upon the Pacific coast has been a difficult matter to determine. Prince Rupert, as it has been named, lies 550 miles north of Vancouver, which is at present the nearest railway; and is not many miles south of the Alaskan boundary. The harbour possesses some of the greatest advantages on the coast. It has a direct entrance channel of more than ½ mile width and is of nearly 11 square miles in area, with accommodation for the largest shipping. Preparations are in hand for the building of the town, and wharfage is already constructed. Before many years the terminus will grow up into a model city, being laid out from the outset upon a definite and comprehensive plan, under the undivided control of the railway company.

Here will be developed one of the largest ports in Canada, whence steamers will run direct to China and Japan, the sea voyage to which will be reduced by two days; thus providing the shortest route between Europe and Asia.

This is rendered possible by the terminus being more northerly than any other existing port, the Pacific coast trending in a north-westerly direction, and thus reducing the length of the sea passage. The Grand Trunk Pacific and the Canadian

Pacific Railways will together provide a fine tourist route through Canada, travellers being able to go out West by one line and return East by the other; the wonderful coast scenery between Prince Rupert and Vancouver being negotiated by steamer.

It may be doubted whether many of those who use this railway, and admire the unequalled wonders of the stupendous scenery through which it passes, will think often of those engineers who took their lives in their hands in order to locate and construct it. A member of this Society is engaged upon this work, and the following facts communicated by him may not be out of place.

Owing to the urgency of the case the pioneer and location work has had to be carried out through the long winter, notwithstanding the rigours of the climate. For months the survey party was completely cut off from all trace of civilisation, having to depend for food upon provisions previously hidden in "câches" by the Hudson Bay Company's Traders. These were not always to be found, when actual starvation stared the party in the face, it being impossible to send several hundred miles through the trackless forest to headquarters for food. No communication could be held with the outside world as no letters could be delivered, and only when a stray Indian party was met with, was there any opportunity of sending news to civilised quarters.

In the summer-time there were dangers from accident or disease to be faced, there being no doctor with the party, and malarial risks and incessant discomforts from mosquitoes and other flies were prevalent. So numerous were the latter that nothing, however offensive, rubbed on, would keep them from the surveyor's face, and it was necessary to wipe them off with the hand every time the transit was used.

At times the surveyor had to be roped to a tree, or suspended over a lofty cliff, in order to set a transit point, and progress was so impeded by the fallen trees of many centuries that often only a few yards could be run in an hour. In one case, after three months had been spent in carefully locating a loop line, the work was stopped, and run again with a slightly modified grade, thus eliminating two tunnels through solid rock, 1600 feet and 900 feet long, and making the line 2 miles shorter.

The work was diversified by frequent forest fires, which would sweep down with appalling suddenness and encircle the camp with flames of raging fire, leaping up to 150 feet high, volumes of smoke obscuring everything in gloom, while an intense heat enveloped all; and only after an unremitting fight of sometimes three or four days and nights could safety be secured.

In the winter the cold was intense, the thermometer falling to 64° below zero [as contrasted with 114° in the shade in the summer]. Thus, although a stove was burning in the tent all night, the water in a pail alongside the sleeper would be frozen solid, and moisture from his breath would congeal into an icy lump on the blanket. When the survey was done the party had to tramp back through the snow-clad country for a month, carrying their instruments and food, as the horses gradually died; and after suffering the greatest risks and hardships, they just managed to struggle back to safety, after a final day's tramp of 45 miles, with a temperature of 40° below zero, with nothing to eat; everyone of the party being severely frostbitten.

Thus is rapidly coming into existence this great undertaking, which will open up to civilisation the vast agricultural tracts of the North-West, where there are enormous areas of virgin soil well adapted for wheat growing, and where the timber and rich mineral resources of all kinds only need the advent of railway communication for their development; thus providing new fields for the external growth of the Empire. At present, although the journey across Canada occupies five or six days, yet the whole population does not exceed that of London.

The first of these trans-continental railways was the Canadian Pacific, completed in 1885, and wonderful has been the consequent development of the country; the Grand Trunk Pacific will as successfully open up a more northerly area across the Dominion, at present untouched and undeveloped; and it may be confidently predicted that the time will come when, yet further north, railways will be projected, opening up yet newer districts to the civilizing influence of the human race.

In my former Address reference was made to the scheme for a tunnel between England and France, which had recently been prominently before the world. A commencement had been made and successfully carried out for upwards of a mile under the sea from either end, when political and military considerations put a stop to the work.

Last year the project was revived, the English engineers being Sir Douglas Fox and Partners, in consultation with Mr. Francis Brady, the engineer of the Channel Tunnel Company, who prepared the necessary plans, sections and estimates, in order to obtain Parliamentary authority for proceeding with the work.

It is evident from these, and from the exhaustive reports of the engineers of both nations, that no serious engineering difficulty was to be apprehended in the successful carrying out of

this great work in the course of seven or eight years, and every precaution for the protection of either country from the unlawful use of the tunnel was amply provided for.

After considerable delay political and military considerations again put a stop to the project, and it is worthy of note that no opportunity was afforded for proper consideration of the scheme from the engineering point of view. It is, however, evident that, had the opportunity been provided, the engineers would have demonstrated their ability to triumph over all difficulties. In the words of their report: "Summing up the engineering questions relating to the proposed tunnel, we agree with M. Sartiaux and Mr. Brady in the opinion that the enterprise is one that can be carried out with certainty, and at comparatively moderate cost; the geological and other conditions being of an exceptionally favourable character for the construction of a submarine tunnel."

We may yet see this grandest of all engineering conceptions brought to a successful issue.

Simultaneously with the enforced abandonment, for the time, of the Channel Tunnel, we have seen the revival of another old idea, advocated by Sir John Fowler and other engineers, for better linking up the two friendly nations; the "Channel Ferry" scheme having recently received Parliamentary sanction.

Year by year it is becoming increasingly evident that some scheme of cross-channel communication, whereby unbroken through transit may be provided, is urgently needed. In the case of passengers it cannot fail to be of great advantage that they should be able to travel in a through carriage from, say, London to Paris, or further; thus avoiding the frequently unpleasant experience of embarking and disembarking at Dover and Calais.

This is often looked upon as an exaggerated drawback; but, although much has been done to mitigate the discomfort attending the transfer of passengers and their belongings from train to steamer and *vice versâ*, the process is still far from satisfactory, even in good weather; while under the more usual conditions of doubtful or bad weather during embarkation, which often takes place at unseasonable hours; and the impossibility of securing a restful journey, unbroken by periodical enforced disturbances, the possibility of being able to journey in the same carriage throughout cannot fail to be attractive to the average traveller. It is significant that railway companies are yearly adding to their through trains, which are increasingly

popular; though the discomforts of changing from train to train, under cover of a station and unexposed to the elements, are not to be compared to those which are usually prevalent at a seaport.

The Channel Ferry, however, is not primarily dependent so much upon its passenger as upon its goods traffic. When it is considered that, under the present arrangements, all goods must be shifted at least twice in crossing the Channel, it is obvious that considerable deterioration must ensue; and that not only is there the additional cost of transhipment, amounting in many cases to as much as 2*s.* 6*d.* per ton, but there is also the loss of time to be considered, which in the case of perishable articles becomes a matter of vital consequence.

If goods could be packed at the works direct into trucks, and then sent undisturbed through to their cross-channel destination, it is estimated that they could compete, in many cases, with continental markets, which is now out of the question.

The abolition of the old broad-gauge system in this country, which was just completed at the time of my last Address, was chiefly due to the inconvenience and loss consequent upon break of gauge at points of contact between the broad and narrow gauge systems, and is a cogent argument in favour of through carriage for goods.

The cost of the "Channel Tunnel," including approach lines, sidings, land, engineering and other expenses, is estimated at 8,000,000*l.* for the British half, or, say, 16,000,000*l.* altogether. The cost of a "High Level Channel Bridge," which has been proposed as an alternative, is given as 40,000,000*l.*; and both of these are to some extent untried methods of crossing such a gulf. The Channel Ferry scheme, however, will be far less costly, involving an outlay of not more than 1,000,000*l.*; it is moreover, by no means of an experimental nature; many such ferries having been in successful operation in various parts of the world for many years. Indeed it should be noted that this country was a pioneer in this method of conveyance; for, nearly 50 years ago, the North British Railway introduced one across the Firth of Forth at Granton, and one across the Tay at Dundee, for the express purpose of avoiding transhipment of goods.

At the present day there are between 80 and 90 train ferries in operation in the world. Of these 8 are Danish; the longest, having a run of 26 miles, carrying with the greatest regularity 4 trains daily which run from Berlin to Copenhagen; the average time occupied in transferring the train to the steamer and getting it under way being 6 minutes. Upwards of 70 of the train ferries are American, several having a run of nearly 100 miles. Some of these vessels have a tonnage of 3500, a length of

425 feet and a width of 64 feet, and are capable of carrying 27 passenger or 42 goods cars; while it should be remembered that, though the conditions which they have to meet are far more trying than would be the case in the English Channel—storms of unequalled violence being of no infrequent occurrence on the Great Lakes, while ice 4 feet thick has often to be contended with—yet it is very seldom that any interruption takes place in the carrying out of the train ferry service throughout the year.

The Channel Ferry scheme involves the building of suitable steamboats for carrying trains quickly and safely across the English Channel; the adaptation of existing harbour accommodation for berthing the boats; and the provision of adequate means for transferring trains from the quay to the boats and *vice versâ*. The fact that these matters are in the hands of our Honorary Members, Sir John Wolfe-Barry and Sir William Henry White, and of M. Sartiaux, the eminent engineer-in-chief of the Northern Railway of France, is a guarantee of their successful accomplishment.

The boats for the conveyance of the trains will be approximately 350 feet long and 60 feet beam, and of great steadiness and sea worthiness, capable of carrying a train load of 600 tons, and fitted with bascule ends for the protection of the trains while crossing. They are to be fitted with triple expansion twin-screw engines of 4000 H.-P., and are to run at a speed of 17 knots; they will be of the single deck type, with complete superstructures fore and aft, under which the train will be sheltered, the shipping and unshipping being done from the bows.

They will provide, for passengers who care to leave their carriages during the passage, all the comforts of a first-class railway station, protected from the weather and well lighted, with waiting, refreshment, and smoking rooms, and platforms for promenade.

It is proposed that the trains shall be transferred to and from the boats by the "Passerelle," or movable bridge system. This consists of a steel structure 300 feet long, hinged at the shore end, the outer end being supported by counterbalance weights, and capable of being raised or lowered as may be required. At the end of this is an independent apron, the outer extremity of which is to communicate with the boat, all the adjustment being electrically operated.

Shortly before the time for the landing of a train the movable bridge will be brought to the inclination necessitated by the state of the tide—which at Dover has a maximum range of 24 feet—and will be fixed in position by an ingenious system of electrical interlocking. The apron will then be raised, and

as soon as the boat is berthed will be lowered on to the end of it ready for the immediate passage of the train. The whole of these operations will be under the direction of one man, who from his cabin in the framework of the bridge will have under his eyes all the safety and controlling apparatus.

It may be mentioned that there is practically no difference in the gauge of the English and French railways, an experimental truck having been run from the South of France to Dieppe and from Newhaven to London. Such slight modifications to the platforms, etc., as may be required in this country, in order to permit all existing rolling stock to pass, will be easily carried out. Similar modifications have been made on the Western Railway of France, which was originally constructed by English engineers; it is probable, however, that special rolling stock for passengers may be introduced.

It is estimated that the quay works at Dover and Calais will cost 400,000*l.*, with an additional 70,000*l.* for the lifts, and the three ferry steamers 400,000*l.*

The British and French Governments, and public opinion generally, being favourable to the construction of the Channel Ferry, the success of the undertaking appears to be assured. It cannot be doubted that the increased speed and trustworthiness of the transit thus provided will tend to a large increase of the traffic between this and other countries. It is indeed almost inconceivable that the goods carried at present between the Ports of Dover, Calais, Folkestone and Boulogne amount to a total of less than 150,000 tons per annum; while across Chesapeake Bay, for example, the annual amount is upwards of 700,000 tons. It is obvious that it must be to the interest of the English railway companies to promote this undertaking, by which their passenger and goods traffic will be so largely increased; and it is to be hoped that they will not rest satisfied with a merely "expectant attitude," but that their united support may be forthcoming, so that the Channel Ferry between Dover and Calais may be inaugurated without delay: its assured success will lead to the opening up of train ferry routes to other ports in the Channel, in Ireland, and other places.

The education of an engineer is of so great importance that I need not apologize for alluding to the subject here.

Engineering has been well defined as "The Application of Common Sense to Raw Materials." Taking this definition as the basis of my remarks, I hope to demonstrate that the training, the evolution, of an engineer is a legitimate and important

branch of our profession; it may indeed be considered the most important, for who but a good engineer is likely to produce good engineering work?

The engineer who desires to produce a special article from the raw material has a long and intricate process to carry out.

He must take the ore, with all its superfluous adjuncts; and by calcining reduce it to a more amenable condition. He must then subject it to the evolutionary action of the blast furnace; where, by the application of a correct temperature and the judicious co-operation of other influences, the comparatively pure metal, freed from most of its deleterious and superfluous drawbacks, may be gathered in the crucible, and drawn off from time to time as required for the purposes of manufacture.

Then, by subjecting the product to the requisite further treatment, he finds at his disposal cast or wrought iron, or some form of steel, which he can proceed to manipulate in the necessary sequence of machine tools; until at last he produces the finished article, be it a Forth Bridge, or a steam cylinder, a Dreadnought, or a steel pen.

Here we may have an application to the raw material of common sense, the outcome of many years' experience of many minds, by which means alone we can hope to achieve our ends, and to produce an ornamental and reliable article upon which we can depend to fulfil its required duty.

Thus, by analogy, we may say that we take the youth who is to become an engineer, the raw material [in no offensive sense], knowing that there the necessary percentage of pure metal may lie at present concealed. We frame his early training so as to remove as much as possible of the unnecessary adjuncts; such as lack of responsibility, and ignorance of the prosaic but important details of spelling, writing and grammatical expression, and of elementary science.

We then place him where, by the judicious surroundings and cumulative influence of carefully combined practical and theoretical training [not the "Sandwich," but the "Cake" system] applied by those who are experts in *both* these matters, and who are competent to study the individual effects of the treatment, we may hope to produce something more nearly approaching pure metal.

Now let us treat this product further, according to our ultimate needs; duly considering what we have got to work upon, and not endeavouring or expecting to produce, as it were, a Forth Bridge from that which is better fitted to become a steam cylinder.

Thus we may hope to be successful in our engineering out-

put. But to commence halfway, or to relax the blast, or let the temperature fall, or to ignore any of the obvious indications of unsuitability during the process, or to expect all the portions of our output to be absolutely identical, would be so fatal to the success of our undertaking, that we cannot imagine it taking place in our ironworks.

Why then should it be so common an occurrence in the production of an engineer? It is evident that the fault does not lie altogether with the raw material, but rather with the lack of common sense which we bring to bear upon it; and it is indeed a serious fact that, day by day, our leading boys' schools are placing upon the market improperly developed material; from which it is either hopeless to expect a product of the highest class, or else at least the process must be much more prolonged and costly than is necessary.

Charles Dickens described one of his characters as having been "educated in no habits of application and concentration. The system which had addressed him in exactly the same manner as it had addressed hundreds of other boys, all varying in character and capacity, had enabled him to dash through his tasks, always with fair credit, and often with distinction; but in a fitful, dazzling way that had confirmed his reliance on those very qualities in himself which it had been most desirable to direct and train. They were great qualities, without which no high place can be meritoriously won; but, like fire and water, though excellent servants they were very bad masters. If they had been under his direction they would have been his friends; but he being under their direction they became his enemies."

Richard Carstone's career was a disastrous failure; and who shall say that what the Master thus pictured for us half a century ago is altered for the better to-day?

Consider but the one detail, already mentioned, of writing correctly and grammatically. This must be insisted upon from the outset, for without it we all know how seriously a man is at a disadvantage, in our own, or in other professions; and it is only with the greatest difficulty that it can be acquired later on in life.

Let us call upon our Public Schools to refuse admission to any boy who is not capable of expressing himself in good English, with good handwriting and good spelling; the Preparatory Schools will soon see to it that these matters are properly attended to at the proper time.

As samples of what I am referring to, let me give the following from an unlimited supply:—

Unreliable spelling.

Sirkul; Athmesphear; Chork; Fiewel; Anthrowsight; Squeback.

Unreliable description.

"The heating serface of a horezontal scylenderical boyler with heavy sperical ends."

"Slip means the water is unstaple. A ship cannot go straight against a fowl wing it is always sliping back."

"Power required for repulsion increases the speed of the cube."

"The superstructure of a bridge is put up by divers, whose dress is made of one piece of indiarubber tube lined with wire."

Unreliable calculation.

Height of atmosphere above the earth, 3 feet.
Elevation of outer rail on curve, 10 feet.
Number of $\frac{5}{8}$-inch spherical bullets in 1 cwt. of lead, ·02.
Velocity of steamer, 13 inches per hour.
Power to be applied to spanner, 6,000,000 lb. or 214,000 men.
Time to bore a cylinder, 43 years.

The above will suffice to show the capabilities of some who have passed through what are called good boys' schools, who have been much handicapped in their pursuit of success in the engineering profession.

Recently the Head Master of one of our great Public Schools has expressed his opinion "that too much time is devoted, in many instances, to the study of Greek, and that science may rightly claim its fair share of educational time." Let us hope that such an enlightened opinion may have other supporters; so that a more rational training may, from the outset, be available for the average boy; who is probably not fitted to shine as a classical scholar, but whose future, and the future of his country, demand that he shall be impregnated, at that age when impressions are most easily perpetuated, with those matters of practical, everyday importance, without which he cannot hope to hold his own in the strenuous life of to-day; far less to take a leading place in the pioneer work of the world.

I say, with confidence, to those who are "educating" our boys in this country, that they have great opportunities which they do not always utilise to the full.

As I have stated elsewhere, "No one would desire to underrate the value of the magnificent school influence and *esprit de corps* inculcated by our great public schools. But the same amount of energy, etc., directed more wisely by those who

teach, as well as by those who control the course of teaching, should certainly produce a far more efficient race of men, and especially of engineers. Without some such modification this country will not easily continue to hold her own in the competition of the nations; but with it she will easily hold, and continue to maintain, the lead."

Before concluding I desire to bear testimony to the great improvement which has of late years characterised the treatment of the engineer in the unprofessional press.

In former years many remarkable statements were met with from time to time. The following explanation in reference to a railway accident, from a leading daily paper of 30 years ago, may serve as an example: "There are two methods of running over a curve. If the driver uses the first of these, he gets up a good pace, and, the moment the curve commences, shuts off his steam, opens his regulator, and so runs round the dangerous corner with a long, steady, easily going stroke. The second method is, as the curve approaches, to shorten the stroke of the piston, clap on full steam, and pass the turning by trusting to the category of chances; as a skilled skater will rush across rotten ice, through which a novice would inevitably break."

Such a description would hardly be possible at the present day, yet it was with somewhat of a shock that we read in a paper recently that the "Forth Bridge with its three great towers, and its centre span of over a mile, are familiar to all"; and that in the Tay Bridge of 1879 "The entire bridge was blown, by a tremendous storm, into the water"; while the Quebec Cantilever Bridge, whose recent lamentable failure called forth these recollections, was described as a "great suspension bridge."

I would also urge that due recognition should be given to the engineer when the inauguration is described of some new undertaking, to which he has probably devoted years of care and responsibility, and which owes its success to his experience and scientific ability. Yet how often do we see the names of the civic authorities, or government or borough magnates, prominently recorded in the daily papers in connection with some great achievement; while the engineer is either ignored altogether, or is mentioned casually later in the proceedings. Instances of this may be found in the press notices of the opening of the Simplon Tunnel and the Great Central Railway; and of the new Egyptian Railway last week.

The great temple builders of old cut their names deeply in the stone of their edifices, and covered them with a cement layer,

in which appeared the title of the monarch under whom the structure was reared; being content to await a permanent, if posthumous, renown. To-day it seems but reasonable that the engineer should at least have an equal share of credit with those who afforded him his opportunity.

Finally, Gentlemen, let me reiterate my opinion that there is no more comprehensive or delightful profession than our own; none that calls for more whole hearted devotion on the part of its followers; none that is greater in its unlimited possibilities, or in its noble object of increasing the welfare of mankind.

We have but to endeavour to realize what life in this 20th Century would be, if everything which we owe to engineering progress were eliminated from it, to get some faint idea of what the world's indebtedness to it includes. And, wonderful as has been its development in the past, who shall say that there is not a yet greater future before it?

I have not endeavoured to prophesy to you in detail; to do so, even in outline, would take too long, even were it desirable. Yet every year fresh fields are opened up to our investigation and enterprise. Turn where we will the engineer and the scientist, working hand in hand, are reaching forth to yet greater achievements. What, for example, may we not expect from mechanical propulsion, on sea, on land, or in the air? Who will set a limit to the size and speed of our great ocean liners, or our internal combustion engines? Who will deny the possibilities of wireless telegraphy, or set a bound to what will be done by the practical application of electricity, about which we even now know so little?

Let us see to it that, while we reverence the past achievements of our forefathers, we do not neglect our own present opportunities; so that we may not hide our inheritance in a napkin, but may hand it on, increased a hundredfold, to those who in their turn will soon be looking back upon that which we have done in our generation; for, as Tennyson has told us,

> "As we surpass our Father's skill
> Our Sons will shame our own;
> A thousand things are hidden still,
> And not a hundred known."

Mr. Diogo A. Symons: Mr. Chairman and Gentlemen, it is my pleasing duty this evening to propose a hearty vote of thanks to the President for his address, and I am sure, gentlemen, this vote of thanks you will all most readily give. The address was a most interesting and instructive production, and will form a valuable adjunct to our minutes. This address, as we know, is not open to discussion, but I should just like to refer to Mr. Wilson's remarks about the Channel tunnel. During the last few months I have had business in France, and in the future will be continually going backwards and forwards. I only wish there was a tunnel. I came over from Dieppe about three weeks ago, and suffered three and a half hours' misery. I have very much pleasure in proposing this vote of thanks.

Mr. Butler: Gentlemen, I have very much pleasure in seconding the vote of thanks. Like Mr. Symons, the proposer, I am also rather regretting that there is no discussion upon the address, since there are several points which one would like to talk about. For instance, with reference to the Channel tunnel, Channel ferry, etc., our President's description, as, I suppose, it was meant to be, of *mal de mer* strikes me as very apt when he referred to it as "periodical enforced disturbances." (Laughter.) One other remark, gentlemen, with regard to spelling, of which we have heard some amusing examples. I remember some years ago, when I was about ten or twelve years old, playing that old game of spelling letters—giving one another groups of letters to make into a word. A schoolfellow of mine gave me four letters, e, s, u, g. Well, I puzzled over them for a long time, and could make nothing of them, and finally asked him what the word was meant to be. He said, "g, u, s, e—goose" !!! I am afraid, gentlemen, I am rather wandering away from the point, but I have great pleasure in seconding the vote of thanks to our President for his address.

The President: Mr. Symons, Mr. Butler, and Gentlemen, I thank you for your kindness and appreciation; it is good of you to have listened for so long to what I have had to say. I hope that we are now starting upon a year which will be a "record" for the Society in the way of successful progress. All that I have now to do is to ask you and all our other members, to support the Council at our meetings and at our visits. The least I think we can do, when an author comes here to read us a paper, is that we should all show our appreciation by being present, even if we do not take part in the discussion. The same remark applies to our visits. I know that during this past year we have inevitably suffered a little, because our President

has not been able to be with us. This year the President, as far as I can see, is not likely to be in China, so I need only say that I shall very much appreciate it, gentlemen, and so will all the Council, if you and your friends will be frequently present, and so help us in our work for the Society during the year.

March 2nd, 1908.

TREATMENT AND FORMATION OF ROAD SURFACES.

By A. J. Metcalfe, M.S.E.
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Introductory.

One of the most important and absorbing questions which is, and has been for some years, engaging the attention of road engineers and road users, as well as of those who have the misfortune to reside near a busy highway, is that known as "dust prevention." The motor car has no doubt been the cause of the present-day importance of this question, and of the prominence of this subject in the public mind. The author does not think it requires any apology from him, when submitting a paper of this kind for the consideration of the Society, as the subject is not only of interest to road engineers, but in a measure it is important to all engineers, as well as to the general public. Before proceeding with this subject it will probably be advisable to indicate how the construction of motor cars might be improved, so as to lessen the nuisance caused by them on our highways.

Motor Car Construction.

It is evident to anyone who has noticed the construction of the underside of a car-body, that there are generally several uneven and abrupt projections, and these, together with the small vertical space between the body of the car and the roadway, are the cause of much of the dust trouble, and in next year's designs of cars, several makers are constructing them still nearer the roadway, which will certainly aggravate this already nearly intolerable state of affairs. Unless something is done immediately in the general improvement of the surfaces of many

of our roads – which is not probable—there will arise a much more serious and strenuous opposition to quick travelling cars than we have known hitherto, resulting in more stringent laws and regulations for the control of this kind of traffic.

The nearer the roadway the body of the car is, the lower will be its centre of gravity, thus when travelling quickly round a sharp corner, or any other quick change of direction, the tendency to overturn will be reduced. To ensure the safety of other traffic using the same roads, all corners should be negotiated slowly, and the alteration in next year's cars will be of little advantage to anyone, but will rather be a source of danger, as these awkward corners will be taken at a higher and therefore more dangerous speed by many drivers, and accidents will probably be the chief result of the alteration.

Road engineers do not wish to be unreasonable or to ask for anything impossible—in car construction—when they ask that no projections be allowed on the underside of cars, and that they be so shaped as to reduce to a minimum, all eddying and scurrying of the air or wind under the car, as the latter passes over the road, for it is generally conceded that these abrupt projections, coupled with the other error spoken of, cause more dust disturbance than tyres.

Some Difficulties Retarding the Improvement of Road Surfaces.

What concerns all road engineers mostly is the fact that present-day roads and methods of repair are, in many cases, utterly unsuitable for present-day needs. During a continuance of dry weather, on nearly all our highways, we are constantly reminded of this fact. One cannot go along any important "through" road in fine weather—that is, any road made without the aid of some bituminous binder—without being nearly choked with dust; and longsuffering though the British public is reputed to be, yet it will ere long demand, in no uncertain voice, a remedy, either by the abolition of the motor car from our roads, or else by the making of the roads more suited to this traffic. As the motor car and other self-propelled vehicles have undoubtedly come to stay, there is nothing for it but to devise some means of ameliorating, if not of abolishing, this trouble, by improving the materials and methods now in use on many of our roads. Whether the recently-reported discovery of Edison will revolutionise the method of propulsion of these vehicles remains to be seen, and whether in doing so the invenon will tend to reduce the dust nuisance, can only be proved

by events ; but road engineers fully recognise that some method or methods of making roads more suited to the change of traffic is indispensable, and a road-surface has to be found which is suited not only to the rapid travelling motor car, but also to the heavy motor lorry, the traction engine, all kinds of light and heavy, quick-moving and slow-moving horse traffic, as well as to the pedestrians. To make a thoroughly satisfactory, durable roadway, at a reasonable cost, suited to the varieties of traffic just mentioned, is no easy matter, in fact many road engineers assert it is an impossibility to do so, and the more one studies the question in relation to the traffic, and the more one experiments both in materials and methods, the more difficult, in some respects, does the problem become, for what is suitable to one class of traffic in a road-surface, is unsuitable to another, and *vice versâ*.

Then having discovered a suitable material, and a satisfactory method to meet the above requirements, it must also be suitable for all kinds of weather, and be equally good in wet, or frosty or other trying weather as in fine dry weather, and equally durable and satisfactory when the thermometer is at 100° F., as when it is at zero ; and these requirements augment the difficulties indefinitely.

Recent Tests and Trials.

We recognise the good educational work now being carried out by "The Roads Improvement Association," and "The National Dustless Road Committee," formed by that Association. Undoubtedly they are assisting us in various ways, especially in the education—by demonstration—of the public to the idea of reform being possible in the construction and maintenance of ordinary highways ; for, in May last, this Association carried out some practical experimental tests on several country roads not far from London, in order to determine (1) The best preparation of tar for road purposes, and (2) The best tar-spreading machine.

For the machines, the tests were carried out on three classes of road, viz. (1) on granite macadam, (2) on flint macadam, and (3) on gravel roads. The first test was made on the Staines and Hounslow main road, the second on the District road between Twickenham and Kempton Park, and the third on a road at Ascot. Seven types of machine entered for the trial of which six competed, one—the Tarmaciser by name—meeting with an accident *en route* to the site of the trials, and thus being rendered unable to compete. The names of the remaining six spreaders are :—

1. Aitken's patent pneumatic tar sprayer.
2. Emulsifier, Limited.
3. Johnston Lassaily patent tar road binder.
4. Tarspra, Limited—Thornycroft's motor van.
5. Thwaite's ante-road-dust system, and tar boiler.
6. Reeson's patent machine.

A few months ago the judges appointed by the Roads Improvement Association issued their Report and Awards, and the first prize of 100 guineas and the Association's Gold Medal was awarded to No. 1, Aitken's Pneumatic Tar Sprayer, and in their Report on this machine they say, "It is a well constructed machine, possessing many advantages, and can be moved rapidly from one part of the road to another. It can also cover a considerable superficial area of road in a day's work. Its construction is solid and well thought out, and the cost of working and maintenance may be expected to be low. The judges believe that by the use of this machine under the most favourable conditions, the actual cost of laying on one coat of tar in a sufficient quantity (not counting cost of tar) may be cut down to almost one-fiftieth of a penny per super. yard, or with a road 6 yards wide to approximately 1*l.* per mile."

The second prize of 50 guineas and the Association's Silver Medal were awarded to competitor No. 4, Tarspra Limited, for 700 gallon Tarspra Thornycroft motor van, which in their Report the judges state, "Resembles in some respects the Aitken machine. The machine worked well and spread the tar evenly. It is well designed, but the workmanship is not equal to Aitken's machine, and probably the cost of upkeep will be greater."

The Tarmaciser machine was tested at a later date, but owing to further accidents, etc., it did not give the result claimed by its makers, and the judges state, "Although there are probably possibilities for this machine, they are of opinion that it cannot at the moment be considered an entirely practical apparatus."

The other machines are commented upon in more or less favourable language by the judges.

In the preparations of tar, for which only one prize was offered, there were seven competitors, the preparations containing in several instances a small proportion of tar, and in others a large proportion.

Those containing a small proportion of tar were:—

1. Craig's Crempoid R. and Crempoid D. compounds.
2. Ermen's Ermenite.
3. Hahnite.
4. Kay's Pulvicide.

The judges in reporting on these four preparations state: "The results of treating these roads with these compounds may be dismissed in a short paragraph. Within a short time after their application, i.e. a *week* in the case of *Ermenite*, a *fortnight* in the case of the *Crempoid* and *Pulvicide*, and a *month* in the case of *Hahnite*, they had practically disappeared. The compounds have not the durability of tar, and the judges do not consider their use economical. Moreover, complaints have been received to the effect, that the dust raised from the sections of the roads treated with some of these materials is injurious to the eyes of those using the roads, and to the frontagers living along their routes."

The preparations containing a large amount of tar, which were entered for competition, were:—

1. Clare's patent tar compo.
2. Oil gas-tar, from "The Gas Light and Coke Co."
3. Marriott's "Marbit."
4. Tar (patent) Solidifying Co.'s solidified tar.
   (The last named did not compete.)

The judges awarded the prize of 100 guineas and the Association's Gold Medal to R. S. Clare, for Clare's patent tar compo; and in their report on this preparation they say: "The section of road (on which it was applied) as regards dryness of surface was approximately in the same condition as the other lengths. The macadam was in good condition, the quantity of the material appears to be 1 gallon to 7 square yards applied in two coats. At the price quoted by the competitor: 3½*d.* per gallon d/d London; and after making allowance for getting the material into position on the roads it works out excluding the cost of spreading for a coating of 7 square yards to the gallon, at 0·57*d.* per square yard of road surface, or approximately at 25*l.* per mile of road 6 yards wide. From the time of laying until recently it has been uniformly good, and is now in better order than any other portion of the road. The smell of the material is relatively evanescent and unobjectionable."

They further state their opinion "that its success is greatly due to the fluidity obtained, even when applied cold as was the case on the Staines Road, the penetration was very considerable. In spite of this the tar has body enough to hold together the small dust-forming particles in a highly satisfactory manner."

The judges place "oil gas-tar" next in the order of merit, and amongst their remarks regarding this preparation state, "The length of road (treated with oil gas-tar) is in a fairly good condition. Oil gas-tar probably penetrates deeper than anything

else, but it does not form so durable a wearing surface, and consequently is not so great a protection to the face of the macadam as a good tar preparation. It would probably be very useful for the purpose of giving the first coat to a road, to be followed by one of coal-gas-tar or other similar substance."

As regards "Marbit" all that the judges say is, "The condition of the section of the road treated is not quite so good as that of the two first named."

The success, and in a measure the reliability of these tests were affected by unfavourable weather, and it is quite possible, given better conditions that other conclusions would have been arrived at.

The Dustless Roads Committee are arranging for further experiments near London, on roads carrying traction traffic, motor waggons, fast motor cars and heavy and light horse traffic, and they intend to experiment with half-mile lengths of road, and to lay a length of ordinary macadam on the same road as a datum, so that the life of each of the other materials tested may be compared. An accurate account of the prime cost will be kept in each case, which will be sub-divided under the heads of materials, cartage, labour, etc. Maintenance charges will also be recorded.

A record of such data will be useful and interesting, but after all, the Dustless Roads Committee are only carrying out in a more public manner, and perhaps on a larger and more varied scale, experiments which many road engineers have been and are carrying out on their own roads, and there is little doubt that the conclusions this committee will arrive at, will, in many respects, confirm those which several engineers already have come to.

Cost.

A further difficulty in the way of reform, is the question of how the extra cost of applying the material is going to be met. This difficulty is, in the author's opinion, the one which is the greatest obstacle for us to surmount. It is no longer a question of no suitable materials or methods having been discovered, the trouble now is largely a financial one. The highway maintenance of this country is administered respectively by County, Borough, and District Councils, and before you can carry out these reforms, you have generally to satisfy these Councils of two things, viz. (1) that the new materials and methods are really better than the old ones, and (2) that the cost will not be any higher, and this latter demand has undoubtedly tied the hands of many road surveyors, and will continue to do so. A few of these authorities are endeavouring to move with the times, and are supporting the efforts of their surveyors to render

the roads under their control more dustless, and therefore more durable, but the great majority are practically doing nothing in the matter. Looked at from their point of view, there is some excuse for their refusal to move forward. When we consider, that our water-supply, sewage disposal, hospitals, asylums, workhouses, relief of the poor, the police, highway administration, etc. (not forgetting that recent and most serious addition to the ratepayers' burdens, viz. the cost of elementary and secondary education), are nearly all financed from the rates, it is almost a wonder that any of the authorities have gone into the matter of road reform, which initially if not ultimately costs more money than the old system. Thus the question of Imperial assistance to highway maintenance has arisen, for it is quite obvious that little more can be done by the ordinary ratepayer. Were the finances of the Imperial exchequer such that 1,000,000*l.* could be contributed annually to the improvement of road surfaces, then the dust problem, as we now understand it, would in the course of a few years largely become a thing of the past on the most important of our highways.

Materials and Methods.

Having indicated some of the difficulties in the way of reforms, we will now direct our attention more to the composition of the materials used, their method of preparation, application and their cost.

There are many materials now on the market, and numerous methods of treatment in use for dealing with dust prevention or palliation, and road preservation, and these methods resolve themselves into one of these classes, viz.—

1. Treatment by spraying of existing surfaces.
2. Coating with ordinary material and using a preservative binder.
3. Coating with materials previously treated with a preservative.

1. Treatment by Spraying of Existing Surfaces.

Amongst the preparations intended for spraying road surfaces there are several which are really only dust palliatives, and some of these we will notice briefly first.

1. *Westrumite* is a patented fluid, and has been before the public for some years, and has met with varied success up and down the country. It is mixed with water and applied to the roadway, thus diluted, with an ordinary water-cart. It requires

the operation to be repeated at intervals, and during a continuance of fine weather some very good results have been achieved, but during showery and changeable weather, inferior results only have been met with.

2. *Akonia* is also a patented fluid, and can be had at a lower cost than Westrumite. It is hygroscopic in its action, and has been, during the past two years extensively used in various parts of the country, and the results whether, good or bad, have been mostly due to climatic causes. Calcium chloride enters largely into its composition.

3. *Calcium Chloride*, like Akonia, has the deliquescent property of making and of keeping, in a greater or lesser degree, the surface of the roadway moist in dry weather. It is mixed with water, and sprayed on the road from a water-cart.

When the weather is very dry and hot, the sun sometimes overcomes this hygroscopic property of both Akonia and calcium chloride with the result that a very irritating dust is formed, which is injurious to the eyes and skin, and thus very unpleasant in breezy or windy weather.

Then there are the preparations partly composed of tar. Amongst which are: (1) Crempoid, (2) Ermenite, (3) Hahnite, (4) Pulvicide, (5) Emulsifix.

1. *Crempoid.*—This preparation is the invention of Mr. A. J. Craig, of Edinburgh, and is made in two mixtures, "Crempoid D" does not contain any tar. It is mixed with oil instead, and is intended specially for spraying purposes. Otherwise the two mixtures are similar, the ingredients being glue and bichromate of potash, and either tar or oil. The mixture containing tar is called "Crempoid R." Its maker claims that no offensive smell is caused, it has no chemical action on varnish or rubber, does not produce slipperiness, penetrates further into the road than ordinary coal-tar, and acts as a disinfectant. It has been used mostly in Scotland, but its success at the trials previously referred to, was very short lived, due no doubt to the unsuitable weather.

2. *Ermenite.*—This is an invention of Mr. W. F. Ermen, of Manchester, and is a mixture of cotton-seed oil treated at a high temperature with sulphuric acid. This product, after being washed, is mixed with four times its weight of crude tar. The mixture is further treated with hot caustic soda, and agitated until thoroughly emulsified, and afterwards diluted with water until it contains 20 per cent. of tar. Thus it is a soapy solution containing emulsion of tar and a little free oil. Its price is 10*l*. per ton, and it is said that 1 ton will cover a road, 6 yards wide, 1 mile in length. At the trial in May, however, it proved a failure, due no doubt largely to the adverse weather.

3. *Hahnite* is a patent liquid, insoluble in water, and is composed of asphalt, tar, carbolic acid, oil, etc. The basis of this preparation is oil and water—the oil remaining unsaponified. The mixture is laid on the road by the aid of water, the result being that the water evaporates, leaving the Hahnite on the road, and this oxidation makes the road surface waterproof. The makers claim that it has excellent binding properties, gives the road a smooth (but not slippery) surface, is dustless, has the colour of asphalt, and that rain has no action upon it after it has been thoroughly fixed in a dry roadway. It is also, they say, effective on all kinds of surfaces, does not injure rubber tyres or varnish on vehicles, and one dressing will last from four to seventeen weeks, the duration depending on circumstances.

Evidently the "circumstances" were not of the best at the competitive tests, as Hahnite had disappeared in a month after application, thus lasting four times as long as Ermenite, and twice as long as Crempoid and Pulvicide.

4. *Pulvicide.*—This is the name given to a preparation of Messrs. Kay Brothers, Limited, Stockport. It is a compound consisting of (*a*) coal tar creosote, 50 gallons; (*b*) coal tar pitch, 1 cwt.; (*c*) resin, 2 cwt. 1 qr. These are mixed together and melted until quite dissolved. Caustic potash or caustic soda (28 lb. by weight or thereabouts), is dissolved in 18 gallons of water, poured into the compound and agitated until thoroughly blended. The result is an emulsion, which, when sprayed on the road, is said to thoroughly bind the road surface as soon as the water has evaporated.

It has been used in many places with varied success.

5. *Emulsifix.*—This is prepared by Emulsifix, Limited, 55 Cross Street, Manchester, the chief component being *oil of tar*. It is mixed with water mechanically, forming an emulsion which is spread over the road, and when the water has dried up, it forms a tarred surface. It is spread by means of a cart called the "emulsifix" cart, which is fitted with two tanks, one for holding the tar oil and the other the water. The tar oil and water meet in a box provided with quickly revolving blades or fans which emulsify them and press the mixture through a pipe on to the road.

It has been used at several places near Manchester with very fair results, about a mile of road being treated in each instance. Two applications were given and lasted from a fortnight to three weeks, and about eight applications per annum are required. The cost is said to be 15*l.* per mile per annum.

We will notice next, those preparations chiefly composed

of coal tar or other bituminous substances, amongst which we have (1) Tar Compo, (2) Marbit, (3) Dustabato, (4) Solidified Tar, (5) Oil Gas Tar, (6) Tarvia.

1. *Tar Compo.*—We have already stated that this preparation was awarded the Prize and Gold Medal at the competitive tests.

It is a purified coal tar, and so treated that it is said to be as efficacious in winter as in summer. In consistency, it is very thin, and can be applied without previous heating. It oxidises in from 5 to 15 minutes, according to climatic conditions, and after this takes place, it should be dusted over with clean grit. It is nearly without smell, and is said to be harmless to fish, and non-injurious to tyres, horses' hoofs, and varnish on vehicles.

2. *Marbit.*—This preparation, the invention of Mr. T. G. Marriott, of Glasgow, competed at the trials, but did not give as good results as the others. It is composed of coal-tar, and a special form of natural bitumen. It is hot when applied to the road surface as a rule, and in those trials, a gallon of the preparation covered about 13 square yards in one instance and about10 square yards in another, but it seems a material more suited as a binder for new coating and rolling than for sprinkling or treating an ordinary road.

3. *Dustabato.*—This invention is prepared by Dustabaters, Ltd., Hartmann Road, Silvertown, E., and there are two preparations of it called "grades," one for surface treatment, and the other for roadmaking. Regarding the former grade, the makers claim that it can be applied by ordinary water-carts, is always ready for immediate use, and is binding, preservative and germicidal. It is claimed also that it will mix well with water, that each successive application tends to further improve the surface, and that the cost is no higher than ordinary watering.

We will notice the binder grade later.

4. *Solidified Tar.*—This invention is prepared by the Tar (Patents) Solidifying and Distilling Co., Ltd., Queen Victoria Street, E.C. About 4 per cent. of the light oils are extracted from the tar, and afterwards solidified, and concentrated, thus reducing the cost of transportation. It does not, however, seem to have been much used for road purposes.

5 *Oil Gas Tar.*—This oil in coal-tar is that which coagulates last, and remains a liquid, even at a low temperature, thus requiring no heating previous to application, and being insoluble in water. At the competitive tests, it was placed second to tar compo as a material for surface treatment. When placed on the road, it does not remain sticky, and traffic can use the treated road very shortly after operations. It is said to be a powerful disinfectant, is quite inoffensive and harmless, nor does it make the road surface slippery, and is not very inflammable. Its cost

is less than ordinary gas tar, varying from 1*d.* per gallon at Southall to 2½*d.* per gallon at Slough. Considerable lengths of road have been treated with this preparation at Southall, the average cost of the finished work being only ¼*d.* per square yard per single dressing. At Slough, the same satisfactory results have been obtained as at Southall, the cost of the finished work of the first coat applied being about ½*d.* per square yard, and of subsequent coats, about ¼*d.* per square yard. On the first dressing, 1 gallon did about 8 square yards, and the second coat about 16 square yards. The advantages claimed by the use of this material at Slough are—(1) it is easily applied without interfering with traffic, (2) no objectionable odour, (3) road surface made practically impervious, (4) no disintegration of surface by motors, (5) very little mud in winter, (6) less wear of surface, (7) less noise. Seven very useful qualities. At the competitive tests, a special machine invented by Mr. Reeson for the purpose of spraying this oil tar on the road, was exhibited (but did not compete) by the Gas, Light and Coke Company.

5. *Tarvia.*—This preparation is obtained from coal-tar, and after being heated from 160° F. to 180° F., it is applied to the road surface either by hand or machinery. If the road crust be dry and the weather fine, and all dust removed, 1 gallon will cover from 2 to 3 square yards, and will penetrate 2 inches into the surface. Tarvia has been used more as a binder than for spraying, and as a binder will be referred to later.

Total Cost of Spraying.

Assuming that we have 27,000 miles of main roads in England and Wales, on an average 6 yards wide, and taking oil of tar as a basis of calculation, and assuming further that the average cost of this liquid, and its application works out at 3½*d.* per gallon, and that for a first coat a gallon would cover 8 square yards, and for the second, 16 square yards, then the first coat would cost 20*l.* per mile or 540,000*l.* for the whole of the main roads, and the second coat would cost 270,000*l.*, or the two coats 810,000*l.*

This figure does not include any of the district or subsidiary roads, and some of the district roads have to carry almost as much traffic as the main roads. Assuming that one-sixth of these roads were treated, there would thus be an additional mileage of 18,000 to allow for, which would cost for the first coat 360,000*l.* and for the second coat 180,000*l.*: or together, 540,000*l.*; and for the main roads and one-sixth of the district roads there would be required 1,350,000*l.* per annum at least, even if this low-priced method of treatment were used.

2. Coating with Ordinary Materials and Using a Preservative Binder.

This method of treatment has been extensively followed at various places, and on the whole very satisfactory results have been achieved. The roadway is coated in the ordinary way with granite, quartzite, basalt, or other hard material, which is well rolled and closely keyed together whilst it is thoroughly dry. Hot tar is then applied either by watering-can or from a cart suitable for the purpose, and the tar is spread evenly over the surface by means of fibre or other similar brushes, and a period allowed for it to percolate into the interstices. Half to one quarter inch screenings of a material similar to that used for the coating, are spread evenly over the tarred surface, and all thoroughly rolled in and consolidated. If the roadway is in fair condition and of correct camber, a coating one stone in thickness will be sufficient to render the surface quite waterproof. This will cost no more than a coating of ordinary macadam two stones in thickness, and yet it will wear as long, and cause much less trouble from dust, as well as less cost in scavenging and surface labour.

A continuation of fine weather is an absolute necessity if satisfactory work is to be carried out by this system, for it has been found repeatedly, that if either the roadway contains too much moisture, or the weather is too wet or damp, the results attained have left much to be desired.

As regards the kind and quality of the coal-tar likely to give the best results, it is very important that tar should be selected carefully, and any which contains a lot of water or other impurities should not be used in its crude state. If you have tar containing a quantity of water or light oils, it should be heated until all the water and lighter volatile oils are evaporated, when the hot tar remaining—other conditions being equal—will generally give a good result.

Tests were made of crude and refined coal-tars at the competitive trials, and regarding the crude coal-tar, the Judges stated, "it did not yield a satisfactory result unless applied in a quantity in excess of that found necessary in the case of Clare's Patent Tar Compo; one and a half times the quantity is required to give equal results."

Regarding a "Refined" or "Freed" tar from the same company as the crude tar (the Metropolitan Gas Co.), they say, "This is a coal-tar from which the water and naphtha have been distilled, and which has a specific gravity of 1·24. The distillation serves to remove the ammonia—which is an objec-

tionable constituent of tar for road treatment—and the light oils. The cost is about $\frac{1}{4}d.$ per gallon more than the crude tar. The preparation answered well in the trial."

Thus we see the importance of having tar which has been purified, whether it be intended for surface treatment or for grouting purposes. The consistency best suited for grouting purposes is when the tar becomes sticky, and the mixing ladle, when taken out of the tar and drawn gently upwards until from 2 to 3 feet from the liquid, forms "needles" between the ladle and the liquid. These needles should not break or separate until they are reduced in diameter to about $\frac{1}{24}$ inch, when at a temperature of about 80° F. The heat is then increased until the tar is very thin, and it is applied to the newly stoned road, and the result is a road which soon sets and is ready for traffic immediately it is rolled.

There are several patent preparations specially intended as a grout or binder, amongst which are—(*a*) Dustabato the road-making grade, (*b*) Tarvia, and these two only will be briefly noticed.

(*a*) *Dustabato Road-making Grade.*—It is said that this preparation will thoroughly bind the hardest materials, such as granite, and even when laid under adverse climatic conditions, some of the roads made with this preparation are giving the greatest satisfaction. In East Ham, half a mile of road was laid with this binder, and the surveyor there speaks very highly of its qualities. He puts the cost at 6*d.* per square yard more than ordinary macadam—a somewhat high figure which will prevent the extensive use of this patent material.

The material is delivered on the roadway in solid blocks. The newly-coated road is dry-rolled, and thereby closely packed or keyed, and made to the correct camber. The blocks are then melted in boilers, and applied direct to the road. Chippings, of the same stone as that forming the coating, are spread evenly over the surface, which is steam-rolled, and as the binder is of a very tenacious and quick-setting kind, it soon becomes a thoroughly waterproof, hard road, ready for use. This adhesive property of the binder makes a roadway such that it will resist the heaviest traffic, and prevent any licking up afterwards.

It is claimed to be non-slippery, noiseless, and to give as good results in winter as in summer.

(*b*) *Tarvia.*—This preparation has been described as a bituminous cement because of its high degree of adhesiveness, tensile strength, and impermeability. It is prepared in such a manner as to prevent the contained bitumen oxidising, and it can be heated to any degree almost, without losing any volatile oils contained therein.

A road constructed with this binder, it is claimed, retains its life for a long period, the binder does not dry or peel off the surface, or otherwise perish in a few months. This preparation is now being mixed by its makers—Messrs. Bristowe and Co.—with fine granite chippings free from dust, and is specially intended to be used as a matrix or flux. At Staines, Eton, Winchester and elsewhere it has been used in this way, and we shall specially notice this method of road treatment in a paragraph to itself later.

Tarvia, like all the best binders in use, is a coal-tar preparation, and has been very extensively used in America, where the practice is to heat it to about 180° F. in boilers on the road, and a gallon of this preparation has been found to do from 2 to 3 square yards. Screenings are thrown over the treated road, which is then thoroughly well rolled, and in the course of a few weeks, any chippings which have not been bonded with the Tarvia may be removed and the surface left smooth and clear.

It is often used as a road-dressing and as such it is claimed that one application will last 12 months, and subsequent applications need only be made once in two years. This claim will no doubt be governed by local circumstances, and in some instances will have to be modified. The author has not seen a price list of this material, and cannot therefore compare the cost with that of any other material mentioned.

The Matrix or Gladwell System.

This system, first introduced and tried by Mr. A. Gladwell, surveyor to the Eton Rural District Council, is of recent date, and thus, like several other systems, is in its experimental stage; but it is claimed to be a thoroughly practical system and many very hopeful forecasts have been made by several more or less eminent road surveyors with regard to its future.

The system is to spread over the length of road to be coated a matrix of tarred material about $\frac{3}{4}$ of an inch in thickness, the matrix being made of fine chippings or screenings, on an average about $\frac{1}{4}$ of an inch gauge, mixed with hot tar or other bituminous preparation. At Eton, Tarvia is used as the bituminous preparation. The usual coating of granite, basalt or quartzite is placed over the matrix, and afterwards rolled with a light roller for a time, and later with a heavier one. The matrix is thus squeezed up into the coating, filling up the interstices from the bottom, and should it be that all the surface interstices are not filled up as well, a little of the matrix is spread over that part of the surface and rolled; in this manner a thoroughly waterproof roadway, very hard and durable, is formed.

Mr. Gladwell—in a specification prepared by himself—amongst other provisions specifies as regards the—

"1. *Foundations.*—If the old road is of incorrect contour or if its levels are too high to admit of additional coating, or if it is misshapen, all these inaccuracies of surface should be remedied and the foundation, 'well rolled down,' and kept well brushed in front of the layer of bituminous matrix at the time of laying.

"2. *Preparation of Matrix or Binder.*– Clean granite birds'-eye chippings, dried by heating on a sand-drier to a temperature not exceeding 120° F. are mixed (as one would mix concrete) with a special tar preparation known as 'Tarvia,' which should be heated in a cauldron to 200° F. and thoroughly incorporated with the chippings, or the matrix can be obtained from Messrs. Bristowe and Co. with the above ingredients ready mixed from their Erith works.

"3. *Laying of Matrix and Aggregate.*—On the old road surface, lay a 'cushion' ¾-inch thick of the matrix, say 6 feet in length, and half the width of the road to be surfaced. Then commence laying the aggregate, which may be ordinary clean broken granite, 2-inch to 2¼-inch gauge, and laid in manner following. Commence laying the aggregate about 6 inches from the end of the matrix (unless the work pieces up with existing work), and lay the granite on with suitable forks so as to leave the smaller flakey pieces of granite as well as dust behind as waste. The aggregate should be closely and evenly packed so as to be nearly as possible, but not exceeding two stones in thickness, bring the coating to within 6 inches of the forward edge of the matrix already laid, then lay another strip of matrix half a yard wide and follow on with granite coating, and so on right through the work, this will enable the men to lay both matrix and aggregate without trampling on either.

"4. *Rolling the Aggregate down into the Matrix.*—The work should be commenced at the middle distance of the section to be resurfaced, and a gang of men should be engaged at each end of the middle section so as to keep the roller fully engaged. The best results in rolling under this system will be found to accrue by the use in the first instance of a roller not exceeding 6 tons in weight; the object being to entice rather than force the bituminous matrix upwards so as to fill the voids in the granite aggregate, the roller should be carefully driven at the lowest speed (at first). After a few journeys over the surface it will be found that the matrix is working upwards; then 'salt,' not cover the surface with a sprinkling of the matrix, brushing this into the voids, roll again and keep on feeding the voids until fairly well filled.

"5. *Surface Sealing.*—Heat 'Tarvia' liquid cement in a caul-

dron to about 300° to 350° F., when it becomes very fluid, and immediately flow this heated liquid over the surface already rolled by means of ordinary watering-cans, having a special V-shaped lip outlet, using about $\frac{1}{5}$ gallon per super. yard. This operation will have the effect of permanently sealing the surface, and if dry granite chippings are immediately sprinkled over the surface flux, the traffic may be at once turned on the road. The object in leaving the matrix about 6 inches forward of the aggregate at the completed end or ends, is, that when the roller goes over it, the end of the work is feather-edged, and thus avoid an abrupt joining.

"Do not allow any sand, earth or other foreign matter to get between the matrix and the granite aggregate or the old road surface, keep both the aggregate and matrix clean.

"Do not attempt to surface flux the work in wet weather, the surface must be dry before this operation is carried out."

The above is a copious extract from Mr. Gladwell's specification, which shows clearly the system, and how it is to be carried out, if success is to be achieved.

The cost of the work is from 3*d.* to 4*d.* per square yard more than ordinary granite macadam, or on an ordinary road of 6 yards in width, it will cost from 130*l.* to 175*l.* per mile extra, say on an average 150*l.* per mile extra. Thus, for the whole of the main roads of England and Wales, 4,050,000*l.* would be required to do the work by this system, and for one-sixth of the district roads, an additional 2,600,000*l.* or nearly 7,000,000*l.* extra to ordinary coating. Although this system works out at less cost than several others, its price is too high to guarantee its being used extensively.

The *result* is yet to be looked for, and some years yet must elapse before any reliable data as to the "life" of this kind of road, the cost of surface labour and maintenance, etc., can be obtained and tabulated.

3. Coating with Materials Previously Treated with a Preservative Binder.

There are several materials and methods in use under this system—limestone, ironstone slag, flints, granite, etc., being the most common aggregates and coal tar, or some other bituminous preparation is the matrix used for treating it. The general matrix used is coal-tar, more or less refined, and these are all known by the name of tar macadam. Taking *limestone* as an example, we will notice one of the systems in use. The stone is broken and screened, into three or more grades or sizes, the largest being from $1\frac{1}{4}$ inch to 2 inches diameter; the second

being from $1\frac{1}{4}$ inch to $\frac{1}{2}$ inch; and the third, all below $\frac{1}{2}$ inch, excepting the fine dust which is better left out, and these are heated on an iron floor prepared for the purpose, until all moisture is driven off.

The tar is placed in boilers and heated sufficiently to drive off all moisture and the volatile and lighter oils, and until it is of a satisfactory consistency, i.e. when "sticky." It is mixed whilst hot with the heated stone, and shovelled over and over until every side of each stone, forming the aggregate is coated. The quantity of tar per ton of aggregate, varies according to the porosity and fracture of the stone, but on an average about 10 gallons per ton is sufficient for the coarsest material, and 12 gallons for the finest.

The tar macadam should be allowed a period to "season,"—three months on an average will do. This makes it not only better to handle, but its properties for roadwork are very much improved, as it gets *tougher* the longer it "seasons." With regard to the *roadway* intended to be coated, the work necessary in preparing it to receive such coating depends largely on the condition and shape of the road. Should it be of wrong shape or camber, then either by levelling down or filling up—as the case may require—the roadway is brought into shape and rolled until quite compacted.

A *camber* of 1 in 40 to 1 in 48 is quite sufficient for tar macadam surfaces. Some of the failures of this description of surface are due to the excessive camber (from 1 in 20 to 1 in 28) which the author has seen on certain roads, causing lateral weakness, tendency to side-slip and inducing the traffic to keep too near the centre of the roadway, thus making it wear away much more quickly in the centre than it otherwise would do.

The camber then of the formation or foundation should be not more than 1 in 40, and at such a level as to allow the specified coating of tar macadam to be put on and rolled in order to bring it up to the finished level. Supposing the finishing thickness of the coat was to be 3 inches, then you would have to put on 3 inches of $1\frac{1}{4}$ to 2-inch material which would roll down to $2\frac{1}{4}$ inches in thickness on an average, $\frac{3}{4}$-inch of the second size which would roll down to nearly $\frac{1}{2}$ inch, and the remainder of finer material. Many engineers now are not using the finest material for surfacing the road work, as its use tends to make too smooth a surface, and to give no grip for horses' feet. A 6-ton roller is preferable to one any heavier for rolling tar macadam (the motor rollers by Barford and Perkins, of Peterborough, being specially suitable for the purpose). After rolling until thoroughly compacted and solid, spread a little clean grit over the surface and the roadway is then ready for use.

Local prices for material and tar control the cost of the work, but on an average, the cost of a 3-inch crust will be from 1*s.* 9*d.* to 2*s.* per square yard, or taking a 6-yard road, it will be from 10*s.* to 12*s.* per yard run, and 880*l.* to 1000*l.* per mile run. This cost is heavier than that of ordinary granite macadam, by about 250*l.* per mile run or 6,750,000*l.* for the whole of the main roads in England and Wales. The use of this material for general purposes, is therefore put out of court.

Ironstone slag has been and is being extensively used as an aggregate for tar macadam, and the process is similar to that already mentioned of treating limestone.

Granite has not given as much success hitherto, when used with a tar binder, as some of the softer materials which are generally more porous, the smoother fracture and cleavage, combined with its greater density and lack of porosity, tending to prevent that cohesion and adhesion necessary to make a road crust capable of standing all kinds of traffic.

The more granulated granites which show a rougher cleavage, should be suitable for tar macadam, and if the tar is of the correct and suitable consistency, it is the opinion of the author, that this kind of tar macadam will, in the future, have much greater success than it has had in the past.

Tarmac.—This material is a mixture of the best selected ironstone slag, and refined coal-tar, and is obtainable from the Tarmac Syndicate. Other makers have a similar material on the market and give it generally the same name, viz. "Tarmac." Whether the materials or the processes are the same, or whether the same care is taken in the selection of both the slag and the tar, is in some cases very doubtful, though the tarmac supplied by one or two of these well-known firms is of a first-class description. As, for instance, that supplied by Messrs. Constable and Hart, some years ago on the Thames embankment, gave a better result than any of the other tar macadams laid. It is still laid there, and was, not very long ago, no worse than on the day it was put down.

The county surveyor of Nottingham (E. Purnell Hooley, Esq. (M.I.C.E.) has for some years taken a very prominent interest in the question of making roads, not only to prevent dust formation; but also to carry heavy traction traffic during all kinds of weather, and from his last annual report his method of preparing and using his tarmac is as follows: "The *densest* slag is used as it comes from the furnaces, broken whilst hot. The tar is heated in boilers, and the two are brought together in a mixing cylinder (patented by himself) both quite hot. The oil of tar penetrates into the slag, and so impregnates it that it will not take up any more moisture from the atmosphere whilst

on the road. This mixed material is allowed to 'season' and 'toughen' for a period, and then spread over the road in sizes of 2 inches diameter chiefly, with a very small quantity of finer material as a surface dressing, and consolidated in the usual way. The less there is of the finer material the better, as the larger stones offer a better grip to horses' feet than the finer ones. Mr. Hooley gives instances of certain roads in his county where they have to be coated with granite nearly every year to keep them in repair. Sections of such roads have been coated with tarmac, which he forecasts will last from 5 to 7 years, that is, 5 times as long as the life of granite macadam, and even on the assumption that it will only last *twice* as long, he shows by the following figures how much the cost is in favour of the use of tarmac.

The figures are as follows :—

	Granite, 2 Years. £	s.	d.	Tarmac, 2 Years. s.	d.
Materials per ton on an equal length of 425 yards run or 3825 square yards	1	0	0	11	10
Team labour		1	1		6
Manual labour—including scavenging, sweeping, etc., for three weeks ..		9	6	3	6
Steam-rolling		1	6½		4
Total	£1	12	1½	16	2

These figures show the granite length to be costing in 2 years twice as much as the tarmac length.

A very striking feature of the above figures is the low cost of manual labour for 2 years on the tarmac length compared with that on the granite road, and the lower cost of rolling also is very striking. The former comparison is of great significance in that, not only does it represent the lessened cost, but the lessened amount of inconvenience and nuisance on tarmac roads, such requiring less than half the scavenging necessary on granite roads.

Some hold the view that this form of road is only suited to light traffic, but Mr. Hooley asserts, that it is the only one which satisfactorily sustains heavy traction traffic in all weathers.

Undoubtedly for heavy traffic the larger the aggregate, the stronger the road, and where constant traction traffic or heavy motor lorry traffic obtains. Materials even up to 2½ inches or 2¾ inches should be used to a depth of 6 inches or more with a surface coating of finer material.

In the author's opinion, the reason why some engineers have such a poor opinion of tarmac as a material suitable for heavy traffic, is, that they have only used materials of low gauge and

probably the crust of the road has not been made of the thickness necessary to sustain such traffic.

The cost of tarmac roads is difficult to define, and we can only put down an approximate figure. For a 3-inch finished coat, the cost will be about 1000*l.* per mile of a roadway 6 yards wide, and if it is made to withstand heavy traffic, the cost will be about 1800*l.* per mile. To put into repair the whole of the main roads in England and Wales, a 3-inch crust will cost 27,000,000*l*, and a 6-inch crust 50,000,000*l.* sterling.

Supposing the average life of a tarmac road was 12 years and that it was necessary to construct $\frac{1}{3}$ of the above mentioned roads with a 6-inch crust and $\frac{2}{3}$ with a 3-inch crust, then the annual cost on a 12 years' life would be nearly 3 millions, a prohibitive figure even on that calculation.

Conclusion.

Thus it will be seen that something is being and has been done in attempting to deal with this modern problem, the chief difficulty of which is a financial one. Let the means be provided, give the road engineers a free hand, and the problem will soon be solved, and the work carried out in an effective manner.

The author desires to express his indebtedness to Mr. Hooley, Mr. Gladwell and others, for placing at his disposal some of the information contained in the paper.

DISCUSSION.

The President moved a vote of thanks to the author for his paper, and, in doing so, remarked that nearly two hundred years ago another Metcalfe had laid out many of our early roads; other engineers had carried on the work, and the roads in this country, if looked at as a whole, would bear comparison with any to be found elsewhere. The author had told them that the motor car had "come to stay," and there was no doubt that the question of road surfaces had in consequence been recently forced upon the engineer, who was doing his best to deal with the matter scientifically. The interesting paper of to-night, and the costs given with it showed that much had been already done. The solution of the question, and the survival of the fittest, would require time; for the problem was not an easy one. They would remember that Mr. Worby-Beaumont, their Past-President, in his inaugural address a year or two ago, had given the Society some very striking figures on the subject. They could not anticipate, at any rate in the near future, a time when

there would be no horse traffic; and therefore the problem of supplying a road surface which would meet all the different kinds of traffic would continually be before the engineer. There might in the future be less of the pounding of road surfaces by the hoofs of horses, giving a heavy blow upon a small surface; but they could not look forward to the time when there would be nothing but motor traffic; otherwise, the problem would be very simple. The meeting would much appreciate the lucid way in which the author had put the facts of the case before them.

The vote of thanks was accorded by acclamation.

Mr. E. F. Spurrell said that the paper was one which appealed more particularly to county engineers than to municipal engineers. As municipal engineer of a London borough, he had no long and wide county roads under his charge. All his roads were paved, but he had always taken great interest in everything appertaining to roads, whether within the Metropolis or elsewhere; and he was of opinion that the preparations mentioned by Mr. Metcalfe did not, to a large extent, preserve the roads. They made them less dusty, and the scavenging bill was considerably reduced; but, all the same, he thought that the preparations put on by tar-spraying machines formed merely a thin skin, which, in a short period, became worn off.

He should more particularly like to deal with the third part of Mr. Metcalfe's paper (page 40), which dealt with "Coating with materials previously treated with a preservative binder." In the Borough of Holborn he introduced tar macadam two or or three years ago, and he had found that, up to the present, it had answered exceedingly well, and he was getting his council to adopt it in other streets. It was a material very suitable for residential streets. It was used by him on a granite macadam road laid many years ago. There had been a complaint that the road cost a lot of money for maintenance and scavenging, and, therefore, he ventured to introduce the tar macadam. It was laid by Messrs. Constable, Hart and Co. Before adopting it, he viewed several roadways in various parts of the country which had been laid with the same material, and obtained the opinion of Mr. FitzMaurice, Engineer of the London County Council. On the Thames Embankment, Mr. FitzMaurice had put down three descriptions of pavement. One was tarred limestone, a second was tarred granite, and the third was tarred slag. The first came up within a very short time. The third, the tarred slag, was still down, and had been subjected to a very fast, light traffic, and it was that description of paving that he (Mr. Spurrell) had adopted within the Borough of Holborn. He had laid the tarred iron slag macadam in two coats. The

bottom coat was to 2-inch gauge, and, when consolidated, measured $2\frac{1}{2}$ inches thick. The top coat was to inch gauge, and measured when consolidated 2 inches in thickness. The slag was obtained from iron works at Northampton, and he specified that it was to be free from honeycomb and dirt, and well dried before mixing. The composition used was tar and pitch, in the proportion of one gallon of tar to $1\frac{1}{2}$ lb. of pitch. On the completion of the laying, the road was thoroughly rolled, and was left $4\frac{1}{2}$ inches thick. Up to the present time it had exceeded his expectations. It was a thoroughly good road, and he was sure that it would be a credit to Holborn. and would reduce very considerably the cost of scavenging and also the cost of maintenance.

Perhaps the remarks which he had made were somewhat in the direction of Mr. Metcalfe's paper, but he thought that dealing with the roads in the manner described in the paper was a very "big order" indeed, and, unless the local authority got assistance from the Government towards the upkeep of county roads, the country would not get the excellent roads indicated by Mr. Metcalfe, and which they were all so anxious to see.

Mr. R. W. A. Brewer said he should like to endorse the remarks of the previous speaker with reference to the usefulness of the information contained in the paper, but he was probably at variance with the author in some respects, as he wished to speak more from the point of view of a road-user than from that of a road-maker. He had used the road for motor cars for the last five or six years, and he had travelled some fifty thousand miles in that way in England and abroad. The paper in a great measure blamed the motor car for making dust. From the point of view of a motorist, he disclaimed that charge. He admitted that motorists raised the dust, but they did not make it. Horses and iron tyres were really the original cause of the dust. The author stated that the sharp projections and the under-run of the chassis had a good deal to do with the raising of the dust. The trials that took place at Brooklands showed clearly that the clean sweep of the under-run to a great extent limited the amount of dust which was raised, and it was rather curious to note that the cars which took the prizes were steam cars. The chassis was rather higher in a steam car than in a petrol car. In the last two cars that he owned, the under-run had been perfectly clear. The engine had been on a metal under-frame, continued from the front end of the engine to the tail end of the gear-box of those cars. Such cars did not raise a very great amount of dust. The author, on page 26, said that the lowness of the centre of gravity in a measure affected the speed of cars in going round a corner. He (Mr. Brewer) did

not hold with that statement in every respect. The risk in going round a corner was not the risk of overturning, but the risk of the car skidding round the corner. He knew what happened to his own car, and he had seen the effect of skidding round a corner on some of the French racing circuits. The speed in the turning of a corner was limited by the tendency to skid, and not by the tendency to overturn.

He would like to refer to the question of the camber of roads, and for this purpose he would make use of the blackboard.

He illustrated how the wide tyres of heavy traction engines could not possibly bed on an excessively cambered road.

What happened was that the road was cut to pieces by the wheels, and he was sorry to say that a great number of the roads on which he had travelled had suffered very much from an excessive camber. Particularly was this the case with Constitution Hill before it was relaid. The effect was most dangerous from a user's point of view, and the road was altogether faulty in construction. He supposed that the idea of a road-maker in constructing such a camber on a road was, that the water should run off the centre to the sides; but, when the road had been in use for a short time, two ruts were formed down the centre, and they held the water, and the road went to pieces "in no time." Of course, in a Tarmac road they got nearer to what they would like to have, namely, a flat road. From a user's point of view a flat road was by far the more economical, because the user could go all over it, and could turn aside without risk to allow anything to pass. Another sort of road was such as was to be found in the main street of Ilford. Whenever he passed along that road, he did so with great trepidation, because the centre portion was paved with sets, and a space of about 18 inches or 2 feet was the limit between the tramway and the edge of the set. A most shocking state of affairs occured where the macadamised road met it. At that point there was a very excessive camber, which was very dangerous when the road was excessively wet.

Of course, a great point about the whole thing was the necessity for a central authority to control the roads. The motorists had done their very best with regard to this, and there had been a deputation to Mr. Asquith with regard to the increase of taxation, to suggest that the money raised should be given to a central authority. It was impossible to expect that the local authorities of this country—a thousand in number, he supposed—could keep their roads in anything like the condition which was absolutely imperative, or would be so in a few years' time. Too much must not be expected from the road engineers. Motorists had done their best to help the road

engineers in the way of money grants. These, he might say, were very small at the present time, but the motorists hoped to get money and to influence money. Of course, they could not afford very much; but, if they could induce the Government to give an adequate grant towards the roads, they would do something which it was worth spending their energy upon. He was very pleased to see the go-ahead way in which Mr. Maybury had tackled this question. It was quite a pleasure to drive down some of the main roads in Kent, say the Folkestone road. He went over them several times last week, and he found them in very good condition despite the bad weather. Motorists appreciated very much the efforts of the county surveyors, and he hoped that they would be helped more by the Government than they had been helped in the past.

Mr. Norman Scorgie said that he had read the paper with great interest and some surprise. Some of the statements of the author gave him many misgivings, and, while he was willing to give credit to the Roads Improvement Association for what they had done in a small way, he was discouraged at hearing the remarks of the last speaker. Having himself driven a car for some thousands of miles, he must say that drivers were a very selfish lot. They expected everything and everybody to get out of their way, and wanted to have the whole road to themselves without any consideration for other traffic. To suggest that a contour of anything like what the last speaker drew on the blackboard happened in actual practice, was misleading.

He took exception to Mr. Gladwell's suggestion that a gradient of 1 in 40, or 1 in 48, was sufficient for a road even with Tarmac. They had to keep the water from lying on the surface of the roads, and he (Mr. Scorgie) would, in macadam roads, give them at least double the camber which was shown.

He did not know the road at Ilford to which Mr. Brewer had referred, but he could quite understand the objection which Mr. Brewer had to that road. There was no doubt that it was an old tramway made up of macadam, which required a certain amount of camber to make it self-cleansing. Now the centre of the road was paved. The authorities of Ilford could not go to the expense of reforming the haunches of the road, so that they had to put the trams on a level as it was, and the result was that, while the centre of the road had a moderate camber for a paved roadway, the haunches, being macadam, must have what he considered an excessive fall.

Mr. Metcalfe had given some very interesting particulars regarding the use of various materials. As far as regards what Mr. Metcalfe called palliatives, he agreed with him, because he had experimented with more than one. He found that they

were effective for the time being, especially those which he described as Akonia and calcium chloride, yet, as soon as winter came they had trouble, and disentegration took place. In addition to this, the very first frost which they got necessitated a sanding of the surface.

Although he was a road engineer, he thought that a good deal of the trouble which was occasioned on the high roads of England was due to the inefficient scavenging of the highways. The traffic would not raise a dust, if the dust was not there to be raised. County surveyors had not the money at their disposal to scavenge the roads two or three times a week, as they were scavenged in boroughs. If they treated a road with tar macadam or any of the palliatives or preservatives, there must still be efficient scavenging. It must be remembered that there were other vehicles travelling on the roads besides self-propelled ones. Unless the horse droppings were cleared away, they went into dust, and caused trouble over and over again.

Some of the figures given by Mr. Metcalfe were exceedingly startling. He told them that the average cost of main road repair had increased from 68*l*. to 95*l*. a mile. That was taken from the mileage that the surveyor had under his charge. But, when he went on to speak of the cost of the palliatives and the preservatives, he reckoned that, with a wizard's wand, the whole could be done in one year, notwithstanding the fact that, if the whole of the roads were recoated in one year, the cost, instead of being from 68*l*. to 90*l*. per annum, would be from 900*l*. to 1000*l*. per annum. Therefore, they must either multiply the smaller sum by ten, or take one-tenth of the other sum.

He hardly thought that the author had expressed his own opinion in the paper. He rather dealt with the opinions that had been expressed by other people. In the first place, he spoke of Mr. Gladwell and the matrix system. If he (Mr. Scorgie) might say so, that was no new system. When he was an assistant in a provincial town, the engineer tried the same thing, with very good results, not on a macadam roadway, but on a set-paved roadway, with a view to the diminution of the noise, He also grouted the sets in with pitch and tar, but the matrix was made as a cush. He thought that Mr. Gladwell's idea was a very good one, because, except on the very hottest day in summer, it was impossible to get tar or any other hot material down to more than $\frac{3}{4}$ of an inch of the face of the stone. What Mr. Gladwell was going to do was to push it up instead of pushing it down. At the same time he was afraid that the cost would be considerably more than was shown in the paper, especially if Mr. Gladwell were to endeavour to remake the

highways of England by half yard lengths one-half of the width at a time.

As to tar macadam, that also was very old. He remembered putting some down more than twenty years ago. It was, in that case, granite which was between, and he could not get the tar to permeate the granite. He did not know whether Mr. Metcalfe had tried how far the tar would permeate into slag, but he thought it would be found that there was only a surface coating after all. Certainly, the tar did not go down more than $\frac{1}{4}$ of an inch.

He did not agree that furnace slag would last five to seven times as long as granite macadam. He had, on more than one occasion, been over to Battersea, where a considerable quantity of tar slag macadam of different sorts had been laid down. Some of the roads had had practically no traffic upon them, except tradesmen's carts. At the time of his visits they looked very well, but one road in particular, an omnibus road, was "scabby" on the surface, and he felt sure that it would continue to be so. He thought that if the ratios were reversed, and slag tar macadam was put down twice as often as granite macadam, one would get nearer the truth. He had made experiments with different materials in London. In London they did not suffer so much from the dust nuisance, because every macadam road in the Metropolis was watered. The cost of palliatives was equal to that of watering the roads, but one could hardly think that road watering would be done upon 27,000 miles of main roads and the many district roads.

While referring to that point, he would ask whether there was not some mistake in the figures given, namely, 27,000 miles of main roads, and 105,000 miles of district roads, or semi-main roads, making a total of 135,000 miles of roads in England and Wales alone.

Mr. Metcalfe said that he had not measured the roads personally, and therefore he could only give the figures which he had obtained.

Mr. Norman Scorgie (continuing) said that he had come to the meeting hoping that they might get some solution of the road question, but he was afraid that the difficulties in the country were more than engineers in town realised. Probably, the experiments which had been made in the Metropolis would tend to help those authorities to whom a penny in the pound did not give so much latitude as it did in London. He thought, with the last speaker, that if anything of a tangible character were to be obtained, they must get some grant from the Government. They certainly could not add to the expenses of municipal government by a direct appeal to the ratepayers of a particu-

lar district, because it often happened that it was not the people who lived in a district that created the nuisance. It was the man who called the tune, like his friend the motor car owner opposite, who should pay the piper.

Mr. A. Gladwell said that he felt that he was in a difficulty, for he did not know whether he should first criticise the paper or reply to the inaccuracies of Mr. Scorgie, who had just sat down. He was greatly indebted to that gentleman for giving him an opportunity of replying to him, and saying that he had never said that a cross section of 1 in 48 was sufficient. He had never gone beyond 1 in 36. He did not know whether Mr. Scorgie had given him the other twelve, or whether they would have to fight for it. At any rate, 1 in 36 was his (Mr. Gladwell's) limit.

He had been very much interested in Mr. Metcalfe's paper. It was a very comprehensive digest of what had taken place in connection with highway matters for some time. Yet he was sorry to see that his attitude of mind was in the direction of the gospel of despair when he said on page 27, "The more one studies the question in relation to the traffic, and the more one experiments both in materials and methods, the more difficult, in some respects, does the problem become, for what is suitable to one class of traffic in a road-surface is unsuitable to another, and *vice versâ*." If Mr. Metcalfe liked to apply himself to the problem of finding a road surface which would be entirely suitable to everything that came on it, he would not have any time left to prepare and read papers on technical subjects.

With regard to the particular method of road making with which his (Mr. Gladwell's) name had been associated, he must correct Mr Metcalfe's statement that the cost was *3d.* or *4d.* more than ordinary macadam. Mr. Norman Scorgie was not prepared to admit *4d.* He (Mr. Gladwell) was prepared to say that on an average it would not cost so much. In a fairly extensive experiment, where the manual labour and carting was properly organised and the materials were reasonably available, the cost of *3d.* to *4d.* a yard would be very much reduced. Mr. Metcalfe made the following absolute statement: "The cost of work is from *3d.* to *4d.* per square yard more than ordinary granite macadam," but an absolute statement like that could not properly be made with regard to this method any more than with regard to any other system in existence. A little while ago he was astonished to find in the case of an authority from the neighbourhood of Aberdeen, that their granite cost them only from *4s.* to *4s. 3d.* a cubic yard delivered on the road. If an engineer got his granite or principal materials very cheaply indeed, it was probable that the bituminous material might cost

a great deal more and be less available than it was in other districts. Therefore, the cost might be considerably more per cent. for a system like this than it would be where the granite was very much dearer, and the tar product very much more available, and consequently cheaper.

He was grateful to the author for having mentioned his name in the paper, and for recognising any assistance he may have given him.

Mr. G. Green said that, as one who had to look after something like 100 miles of roads in the country, he should like to add his thanks to Mr. Metcalfe for his interesting paper. Mr. Metcalfe had exhibited a great amount of patience in the treating of preservatives and palliatives, a large number of which he (Mr. Green) had never heard of before. It seemed to him that, as road makers, they had to look at the matter from an impartial point of view. As Mr. Metcalfe had said, motor cars had come to stay, and road makers ought to be very thankful for them, much as they might distress them, because he believed that the final solution of the road question for the country would be one for which they would be thankful, and one which would be probably cheaper in the end. He believed that the question was not, as Mr. Metcalfe said, one partly of cost, but that it was one entirely of cost.

He thought that he might take it that Mr. Metcalfe's paper spoke of roads generally all over the country rather than main high roads, or even roads in the Metropolis. In his (Mr. Green's) own district, (Wolverhampton) he should think three-quarters of the roads were macadam roads, although they were in a borough. The local authority could not afford to pave them. He had a very small allowance per mile per annum to keep the roads in good condition. He had tried many of the preparations, and he had come to the conclusion that tar, in one form or another, was the cheapest and best material that they could use.

Many road authorities said that a large tax ought to be put on motor cars in order to pay for the roads, but he did not agree with that view. He did not quite see how it was going to be done. If a tax of 20*l.* was put on every motor car, it would not bring in more than about a million and a quarter a year, for he believed that there were some 60,000 motor cars in the country at the present time. That sum would, of course, be a contribution, but, on the other hand, it would probably discourage the making of motor cars, and the manufacture was certainly a national benefit in many ways.

As to the Government paying half the cost of the main roads; that proceeding would not actually relieve the rate-

payers, because the money would come out of another pocket. If it did not come out of the rates, it would come out of the taxes, and he did not think that it would make very much difference in the long run. When it came out of the rates, the authorities would look at the expenditure much more anxiously than if the money was obtained by a grant from the Government, for, when a grant was made by Government, it was looked upon as so much money found for nothing, although the ratepayers really had to pay the money in the end.

Mr. Metcalfe had come to the conclusion that most of the processes described would make the dust-proof roads and the waterproof roads rather too expensive to be adopted wholesale, and the only solution, therefore, seemed to him (Mr Green) to be that they had either adopt them gradually, as he was doing in his own borough by getting an extra grant every year, or find out some simpler method. He had tried one method which he had found simpler and cheaper than tar macadam or any of the other preparations, and that was to make a slag macadam road, to roll it dry, and to grout it with pitch and tar. In his district, a road so made, that is re-coated, came to something like 7*d.* a square yard. This was only one-third of the cost of tar macadam in the same district. He was fortunate in living in a district in which they got slag delivered on the ground at 3*s.* 9*d.* per cubic yard or ton, and he could get tar macadam at about 9*s.* a ton. He could make tar macadam at under 2*s.* a square yard; and a slag-grouted road came out at one-third the cost of the road referred to. This had an advantage which tar macadam had not, and that was that it gave a very much better foothold for horses. The road which he had made that way had not existed sufficiently long for a perfect test, but it is a vast improvement on ordinary water-bound macadam, and has proved satisfactory on an motor omnibus route. In the meantime, while they were adding to their improved roads a mile or so each year in the district, what was to become of the vast majority of the roads in the district? As far as he could see, the only other way was to treat them by one of the other methods—tar-painting or tar-spraying—which had the advantage of making the road temporarily waterproof in the summer, and that lasted, in many cases, well into and sometimes over the winter.

Calcium chloride and some of the other preparations had disadvantages, such as the breaking up of the road surfaces and the production of mud. They lasted a short time in moderately hot weather, but when there was a quantity of rain they became more muddy than before.

Mr. G. W. Manning thanked the author for his paper, and for the large quantity of information which he had embodied in

it. This would be very useful to professional people and others who had roads to make.

With regard to the treatment of the surfaces of roads with tar, that, in his opinion, was what they would have to look to, to a great extent, in the immediate future. He had the honour of being one of the judges in the tar trial. The first prize was given to Clare's composition as the best of that class of material. The report stated that tar had to be laid in a bulk more than double that of Clare's compound, or that class of material, to give an equal result. Clare's compound was laid extremely carefully and with the greatest possible care, great credit being due to the people in charge of the work, and it was in that way that the first prize was obtained. He had no doubt, in his own mind, that if the tar had been spread in the same manner, practically as good a result would have been obtained. Many complaints were raised through the district as to the inefficiency of crude tar. It had been applied by the tar-spraying machine. It was applied in an extremely light coat. He believed that one machine put on a coat of crude tar so thin that only a gallon of tar was used to 18 square yards. This simply wore off at once. As to Akonia and calcium chloride, he agreed with the last speaker and Mr. Norman Scorgie. His council had made experiments with Akonia, and the results accorded with those mentioned by those two gentlemen. With regard to Tarvia, he had used a considerable amount of that material, both as a binder, and as a top surface dresser. A point was made in the paper with regard to this material. It was said, "If the road crust be dry, and the weather fine, and all dust removed, one gallon will cover from 2 to 3 square yards, and will penetrate 2 inches into the surface." He did not know much about the penetration, nor did he believe in it, but the one galloon for 2 or 3 square yards was wrong. With one of the tar-spraying machines, he obtained an excellent coating, which was as good as on the day it was put down, and he got 7 square yards to the gallon.

As to Mr. Gladwell's system, he might mention that it had been his pleasure to collaborate with Mr. Gladwell in that matter, and he had put down something approaching 10,000 square yards under that system. On the first section of road that he put down, he had great doubts in his mind as to the figures which Mr. Gladwell gave. Without saying anything to him, he made up his mind to test the matter, and he, giving the necessary instructions, carried out work covering an area of 4840 square yards. That was with Tarvia binder. He then put down a section of road under the water bind system, making a tar-bound road cover 10½ square yards to the ton and the water-bind cover 11½ square yards. The relative cost came to

1*s.* 6½*d.* for the water-bind, and 1*s.* 9*d.* for the tar-bind, thus giving a difference of 2½*d.* in the cost over the water-bound road. He then tried another section of road covering 11½ yards. This came out at ¾*d.* a yard above the water-bound road; so that the difference of cost in a tar-bound road need not be considered, being very little, if anything, above a water-bound road, if put down properly and with reasonable care.

Passing on to Hooley's Tarmac, he could only say, that here again he had had the best possible results, both with heavy traffic and with light fast traffic. From January to March, 1903, he laid a section of some 600 yards with Tarmac on a main road out of London, and it was to-day practically as good as when laid. The ordinary macadam road at either end was made up at the same time; since then it had been repaired twice, the total cost being 1*s.* 5*d.* per square yard. The Tarmac laid eleswhere gave equally good results. The only objection that could be raised against Tarmac, by those who knew how to lay it, was its initial cost for country roads.

Mr. H. C. H. Shenton said that he wished to raise a question which seemed to have been overlooked. What would be the effect upon streams and rivers and lakes alongside the main roads when the washings from the roads were carried into them? He had heard the point raised by chemists that considerable trouble was to be anticipated from the fish in the trout-streams being poisoned. As the washings from country roads ran into such streams, the point might be an important one, seeing that private owners could obtain considerable damages or compensation from the road authorities where valuable fish were destroyed.

Mr. T. Murphy said that it had occurred to him that the author had perhaps laid a little too much stress on the question of price with regard to the processes. For instance, if they obtained a longer life in the roads, the ultimate cost should not come out at more than an ordinary macadam road, and he believed that, in many cases, it would come out at considerably less. Of course, a great deal had been expected from ordinary macadam, treated with tar, but he believed that the properties of tar were not properly understood at present by many who used it, which was one reason why in one district they got a very good result, and in another district a very bad result, and that was borne out by the statements of surveyors.

Mr. H. Conradi said that he would draw the attention of borough engineers and surveyors to a paper read last week before the Royal Society of Arts by Professor Dr. Hele-Shaw, F.R.S., stating that either asphalt or wood paving supply the best road. He recommends strong binding material, and deep

taring and steam-rolling; while Colonel Crompton, C.B., strongly recommends, besides, the increase of the diameter of the wheels of all motor vehicles. He lived in a district in which he had had considerable experience of motor buses. At first, the companies had about ninety motor buses, running between Cricklewood and the City, and he had studied the question of dust, which was a terrible nuisance. It would be very interesting to find a solution for that public nuisance. He had several times stood for a whole hour, observing the different steam and petrol lorries and private motor cars running along the road, in order to find out the cause of the tremendous dust. He observed that the motor buses at the present moment had too small wheels, and the body of the car should be at a greater distance from the level of the road, especially as there were petrol and oil tanks, gear boxes, driving shafts, and so on, underneath it. While the car was running, the form which the dust took was that of a cone. The dust cone came out at the back of the car, after having been partly formed through the wheel-tyres crushing the surface material of the road. The rapid revolutions cause the fine crushed particles to be picked up by the wheel-tyre, especially where they are grooved, as those of the motor omnibuses. As soon as the centrifugal power overcomes the attractive and adhesive power of the rapid revolutions of the wheel, the dust is thrown up certainly from 6 to 10 feet high, and is spread all over the road. He could assure the meeting that he had gone home like a miller after standing in the summer watching the dust. The road was much frequented by motor lorries and private motor cars, and other traffic. With the electric tramcars going from Cricklewood to Hendon, and from Willesden Green to Hendon, the same thing occurred, but not so heavily. He was not a road engineer, and he believed, from what he observed, that the road from the Crown Hotel, Cricklewood, up to Brondesbury Station was wood paving, well gravelled, well tarred during construction, and now also during maintenance, and the road appeared so far to be well laid and well kept. The dust and noise problem was one of the most urgent for the general public. He was not speaking of the danger which motor buses and motor cars caused through reckless driving, but the deterioration of property along the line of the road from Cricklewood to Brondesbury Station amounted, he should think, to several hundred thousand pounds. He thought that it would be to the interest of borough engineers and of local authorities to study the dust and noise question, and also the construction of roads, so as to prevent the tremendous noise which was going on all day long, caused by the gearing and chain-drive of the wheels, and which was, for

persons who lived along the roadway, really unbearable. The buses began to run between seven and eight o'clock in the morning, and they went on till one o'clock in the next morning. The unpleasantness and the suffering might be understood, when they remembered that something like sixty or more omnibuses were running up and down the road all day long.

Mr. A. J. Metcalfe replying upon the discussion, remarked, that Mr. Norman Scorgie had accused him of not giving his own opinion in the paper. There were a few of his own opinions set forth in the paper, but he had always thought that other men's facts were better than one's personal opinions. The same speaker also said he considered that ordinary granite macadam would last longer than Tarmac. Well, on that point Mr. Scorgie's opinion did not agree with other men's facts. Mr. Scorgie's observations had been unfortunate, and, if he had been doing in Hackney any of this kind of work, he did not want to say he had not done it well, but he was almost led to make that accusation.

As to Mr. Gladwell's remarks on the approximate prices which he had quoted, the only answer was that those prices were statements of fact, and they were also what he considered to be the average prices.

As to Mr. Manning's remarks about the covering capacity of tar and the penetration of 2 inches, he did not think that, if they got a penetration of 2 inches, a gallon would cover 7 square yards of surface; in his opinion, if 3 to 4 square yards were covered, it would be all. Of course, this was qualified by the consistency of the material. The thinner the material, the more it would penetrate and cover. If it was thicker, it would not go so deep nor grout the same area.

With regard to Mr. Shenton's question, he could quite appreciate that, if there was an extra amount of tar used in the aggregate, or more tar than needed, the surplus tar would be washed into the streams. As to the effect, that was, of course, more a chemist's or biologist's question, and it required a chemist or biologist to answer it. He did not know what the effect would be upon the fish or upon anything else living in the streams. Probably, if the stream had been previously polluted by sewage it would be a very good thing, and would act as a deodoriser if nothing more.

He was much obliged to the speakers and to the audience for the way they had received the paper, and for their attention. He did not in reading the paper expect them all to agree with all it contained, and he was glad that it had provoked a little difference of opinion.

(SUPPLEMENTAL REPLY COMMUNICATED.)

Mr. A. J. Metcalfe wrote at a later date, stating that, after fuller consideration of the points raised in the discussion, he desired to make a supplemental reply on the following points.

Regarding Mr. Brewer's remarks he fully agreed that there ought to be more united action between the motor and traction engineer on the one hand, and the road engineer on the other; and could fully understand Mr. Brewer's contention that, on roads of excessive camber, the large flat driving wheels of traction engines, etc., would cut into the road on the inner edges of the wheels and not touch the roadway on the outer edges. The author contends it was here that the motor and traction engineer should do something in the shaping of the wheels to make them more fitted for travelling on the roads, as they all have a camber varying from 1 in 40 to 1 in 18 or so, and, if a slight bevel were given to the wheels, much less harm would be done to the road surface, and the weight would be more evenly distributed.

The author could not agree with Mr. Brewer, who had stated that motor cars made no dust, and that they only disturbed the dust already formed on the road.

When he considered that all the tractive effort propelling the car forward was situated between the road surface and the driving wheels of the car, and that most of the wheels are now steel-studded, or otherwise protected by a metallic device of some kind to reduce wear, there was no other conclusion possible to the person of open mind than that a certain amount of grinding action must be set up between the wheels and the road surface, forming grit and dust.

The same speaker also stated that the liability to "*skid*" is far more to be feared—when turning a corner—than that of *overturning*. Granted, but the author contended that the tendency to skid and also to overturn are very closely allied. The motor bus was a vehicle both liable to skid and overturn; not merely because of its greater weight, or because of any error in the construction of the wheels or tyres, or even in the balancing of the vehicle, but because the centre of gravity of the bus was much higher than in the case of motor cars. Thus the author contended that dangerous corners would be negotiated by many motorists at higher speeds than they hitherto had been, with the probable result of an increased number of accidents.

Mr. Scorgie had mentioned the unsatisfactory condition of the road-surfaces in Battersea made of tar macadam. He (the

author) had seen some of the Battersea roads, and was struck with the excessive camber several of these roads possessed, and this fault was, in his opinion, the chief reason they were not as satisfactory as they should and might have been; undoubtedly there had been failures with this kind of macadam, but they must not allow themselves to be too much influenced by such.

Mr. Scorgie stated that tar did not penetrate into slag. This statement the author maintained was wrong, for he had personally broken a 2 inches diameter piece of Tarmac, and found that the oil-of-tar had penetrated right to the core.

To get this penetration it required the slag to be dry, preferably hot, and the tar at boiling point, and brought together in this state, and thoroughly saturated and mixed.

Mr. Scorgie also stated he could not understand what material per ton in Mr. Hooley's figures had to do with the figure for scavenging. The author considered the statement perfectly clear as it stated a certain area 3825 square yards of roadway. The labour was for a period of 3 weeks, then the comparison was quite a fair one and perfectly clear.

The figures did not show that the price of materials per ton had anything to do with the item of labour, etc.

The author was accused of showing in his figures of the cost, that the cost quoted for the whole of the main roads would be an annual charge. On page 44 they would find he had made one calculation on the basis of a 12 years' life. Where surface treatment was resorted to, which would only last on an average for 12 months, then it must be shown as an annual charge.

April 6th, 1908.

THE DESTRUCTION OF ARCH BRIDGES.

By H. C. D. Scott, M.S.E.

In the following paper the author proposes to deal principally with the methods adopted for destroying old arch bridges carrying public roads over a main line of railway.

The works on which this paper is based, were necessitated through the widening of the L. & N. W. R. Co.'s main line at various places between Preston and Penrith; and on the G. W. R. Co.'s widening between Maidenhead and Didcot.

First the author will deal with the bridges on the Oxheys and Broughton widening, which was commenced in March 1900.

This widening was just over 3½ miles long and cost 50,000*l*., which amount includes 1000*l*. for the removal of old bridges, 28,000*l*. for re-building these bridges, 1000*l*. for extra daywork, 20,000*l*. for muck shifting, drainage, etc., but does not include an amount which was awarded at a subsequent arbitration and which the author is sorry he has not been able to ascertain.

There were eight brick segmental arch bridges to be removed on this widening, seven of which consisted of a single span of about 30 feet, and one bridge, which was in a deep cutting, consisted of a 30-foot arch over the railway, and two side arches of about 25 feet span each. The new bridges had a span of 52 feet each, and were constructed with two main plate girders, cross girders, and jack arches. This design being adopted for its quickness of erection. The removal of the old arches, and other parts of these bridges, immediately over the main lines, was a matter of some difficulty, as the heavy passenger and goods traffic underneath was in no way to be interfered with.

It was finally decided to use explosives and in all cases the arches of these bridges were destroyed by blasting, as this was

considered to be the quickest, as well as the most economical method of dealing with them.

It is proposed to describe the method adopted in blowing up the three-arch bridge mentioned above, as this is the most interesting example, and is typical of the method adopted in the cases of the single span arches.

The three-arch bridge carried a public road in the country, called Lightfoot Lane, over the railway. There is not a large traffic on this road, but provision had to be made for dealing with it, during the destruction of the bridge, and the erection of the new one, by constructing a temporary trestle bridge by the side of the old bridge. The old bridge was built entirely of brickwork, the parapet and spandrel walls being set in cement, but the arches and backing were set in lime mortar.

The arches consisted of two one-brick rings, as shown in Fig. 1, but towards the springing they were half a brick thicker.

The two piers were about 5 feet 6 inches thick each at the rail level, but at the springing level were only about 3 feet thick, the faces and ends being built to a batter of practically 1 in 16. It may be mentioned, though quite a minor matter, that the old work on the L. & N. W. R. was practically all built to a batter of 1 in 16 : but for new works this has been increased to 1 in 12. In some cases it is impossible to measure the original batter, as in the case of a retaining wall at Eamont Junction, near Penrith, which, instead of being battered, was until latterly, overhanging as shown in Fig. 2.

The parapets, road surface, and filling over the backing were removed on week-days and trenches were excavated through the backing over the centres of the piers and over the springing of the side arches: in the latter case the trenches being carried sufficiently deep to expose the extrados of the arches. Twelve holes were "jumped" in the crown of each arch about 10 inches deep; 8 holes in each outside haunch of the side arches perpendicularly over the springing, about 2 feet 8 inches deep; and 8 holes in the spandrel filling over each pier. These holes were from $1\frac{1}{2}$ to 2 inches in diameter, and were driven from the top downwards, being spaced approximately at equal intervals across the width of the bridge. On the morning of the Sunday on which it had been decided to blow up the arches these holes were charged. All the bridges were destroyed on Sundays, as, on these days it was possible to obtain the longest interval between trains.

Of course, no explosive was allowed to be put in the holes until the last train had passed and was signalled "clear" of the block in which the bridge was situated.

The explosive used was "Tonite" consisting of about 54 per

cent. of gun-cotton pulp and 46 per cent. of barium nitrate. It is a high, disruptive explosive, possessing great shattering power, and is moreover unaffected by wet; this latter being an important point, as, when once the date for the work had been fixed, and all preparations made, a postponement, or, worse still, a partial failure owing to damp explosive, would have been most unfortunate.

16 oz. of this compound were placed in each hole in the crowns, 28 oz. in each hole in the haunches over the abutments, and 30 oz. in each hole over the piers; this making a total charge of 94 lb. of Tonite. Wooden rammers were used to drive the charge well home, detonators were then placed in each hole, to the ends of which were attached quick burning fuses (speed about 300 feet per second). These fuses were made of loose gunpowder in a rubber tube. Quicklime was then put into the holes and pressed firmly home, to absorb any moisture, then some sand was put in the holes, and finally the holes were plugged with a lump of soft wet clay. On the G. W. Railway only fine dried engine sand is used for "tamping" and is considered quite sufficient.

The extrados of the arches were then covered with bushes, which were in their turn weighted with heavy iron chains. This last precaution was to prevent the brickwork scattering.

Fig. 3 shows the arrangement of the holes on plan and the grouping of the quick burning fuses, all the fuses from each row of shots being brought together and attached to a slow burning fuse (made of tightly packed gunpowder in a gutta-percha tube and with a speed about 2 feet per minute) the length of this fuse being regulated according to the order in which the shots were intended to be fired. In arranging these fuses great care must be taken to ensure the simultaneous explosion of all the charges in each line of holes, as, if one cartridge was to explode before the others, it might dislodge some of the unexploded ones, and these falling amongst the débris, would be a source of great danger to the men clearing away the débris after the explosion. The fuses should also be weighted down, before being fired, with pieces of brick, or stone; especially in the case of the time fuses, which in burning, sometimes coil back upon the instantaneous fuses and fire them before their time. It has also been found that the instantaneous fuses, unless weighted, are apt to pull away from the cartridge, which then falls amongst the débris, unexploded.

Of course, the instantaneous fuses cannot all be precisely the same length, owing to the central holes in the line of charges being, of necessity, nearer the igniter than those on the extremities, but the difference in time between the explosion of the central charge and the outside one is so small, owing to the

great speed of the fuses, that it need not be considered. In fact, during the destruction of a bridge on the G. W. Railway near Reading, the difference in time was found to be only one fifteenth part of a second.

In addition to the holes described above, each pier had eight horizontal holes as low down as they could be conveniently placed, on the side away from the railway; and about 18 inches deep. Also two holes 1 foot 8 inches below springing level, on the same side, but inclined downwards in order to give a better effect to the discharge; and 2 feet 6 inches deep. The holes in the pier on the widened side of the railway, that is the pier on the west side (see Fig. 4), were, however, not charged.

Both up and down lines had been absolutely blocked on the Sunday on which this work was done, from 12 o'clock noon to 4.30 p.m. for the down road, and 5 o'clock p.m. for the up road.

Immediately the lines were blocked, the rails and sleepers were removed by the company's men. This precaution is always taken on the L. & N. W. R., but on some of the other railways it is not thought necessary. When the lines are not removed the road must be protected from damage by the falling material. This is done by covering the rails with either bundles of straw or stout planks. The latter being the better way as the straw is apt to hinder, to a great extent, the use of shovels when clearing away the débris. The author considers the removal of the lines most satisfactory, as this does away with any risk of the rails being cracked; and the extra time required to relay the road is very small. The slow-burning fuses were then fired simultaneously, their lengths being so arranged that the crown shots of the centre arch should explode first, then the crown shots of one of the side arches, to be followed by the haunch and pier shots of the same arch, the shots in the other arch following in the same order and lastly the shots in the pier.

The explosion of the crown shots crippled the centre arch somewhat, but it still remained standing. After a lapse of about 20 seconds, the crown and haunch shots of one of the side arches exploded simultaneously, with the result that this side arch and the centre arch fell completely. A few seconds later the crown and haunch shots over the abutment of the remaining side arch exploded simultaneously, the greater part of the arch falling, but leaving a mass of overhanging brickwork from the pier, which however, on the explosion of the shots over the top of this pier a second or two afterwards, was blown down. The shots in the base and top of this pier were the last to explode, with the result that the whole pier appeared to lift bodily about 6 inches and then drop down again, but beyond some bad cracks, the pier did not appear to have suffered very much damage.

Within about 15 minutes of firing the fuses, the whole of the arches were down.

A gang of men then commenced to clear the railway of the resulting débris, which in many cases consisted of large blocks of brickwork which had to be broken up before they could be removed. The old brickwork was finally broken small and used for concreting. After $2\frac{1}{2}$ hours work one road was cleared and open for traffic, and $1\frac{1}{2}$ hour later the other road was reopened. There were 95 men engaged on the above work, 74 of them belonging to the contractor, and the remaining 21 being railway-men; the latter only being employed in taking out the rails and sleepers, etc., and relaying the roads after the bridge was down. The cost of blowing down this bridge and clearing the lines afterwards, was about 200*l.*

All the remaining bridges were blown down in a similar manner; the only feature of note being in connection with the single arch bridge carrying a public road, known as Eldon Street, over the railway. About 30 yards to the west of this bridge stands a chimney, 100 feet high, belonging to Eldon Street Mill, the windows in the lower stories of which were boarded up to protect the glass. Some doubts were entertained as to the advisability of blowing this bridge down, owing to the nearness of the chimney. The work was, however, proceeded with, and subsequent events proved the doubts to have been groundless, as the explosion caused not the least harm to the chimney.

The next work to be considered was the widening of the L. & N. W. main line from Clifton and Lowther Station, to Eden Valley Junction, a distance of about one mile.

This widening was commenced in February 1903 at a cost of 8500*l.*; of which amount 150*l.* was allowed for removing old bridges; 3000*l.* for the re-building of same; 100*l.* for providing a temporary bridge at the south end of the works, in order to move the muck on the east side of the line to the spoil ground on the west side; and 5250*l.* for other temporary works, muck shifting, drainage, etc. There was only one bridge blown down on this widening, a masonry segmental arch bridge of 30 feet span, carrying a private road over the railway at the junction of the L. & N. W. and N. E. Railways. Special arrangements were made with the owner, and no temporary bridge was erected. It was decided to blow this bridge down on Sunday, May 10, 1903, arrangements being made to totally block both lines from 10 o'clock a.m. until three o'clock p.m., but, owing to the traffic, the lines were not blocked until 10.20 a.m.

The road metalling, parapet walls, backing, etc., having been removed the preceding week, to the extent shown on Fig. 5, the work of removing the arch was at once proceeded with. Six

holes, 2 inches in diameter, and 10 inches deep, were "jumped" in the crown of the arch, each hole being charged with $1\frac{1}{2}$ oz. of gelignite: the rest of the work being carried on in a similar way to that of the three-arch bridge, already described; the charging of these holes is shown by Fig. 6. After about five minutes from the time the slow fuses were fired, the shots exploded simultaneously, with the result that the arch fell completely, with the exception of a little overhang at each springing, which was easily removed by hand. The abutment on the widened side of the railway, the west side (see Fig. 5) was also removed by hand, during the succeeding week. The work of clearing the débris was in this case helped by the use of a 5-ton crane; heavy masses of masonry being thus picked up, swung round, and deposited on the site of the new line, where it was finally broken up and laid as bottom ballast, 9 inches thick. Barrow roads also being put in wherever possible. By 3.15 p.m. the down road had been cleared and re-opened for traffic, the up road being re-opened by 3.37 p.m. The only delay to traffic was a goods train which was "held" for 20 minutes at Shap station; but probably this would not have occurred, if the traffic department had, as intended, given possession of the lines at 10 o'clock a.m. The only feature of note in connection with this arch, was the comparatively small charge, only 9 oz. of gelignite, required for its destruction. The mortar was slightly perished at the face, but the bridge was perfectly sound.

Very different was the case of a brick arch bridge, of 20 feet space, carrying the approach road to the L. & N. W. goods yard, at Preston, over a branch line connecting the Ribble sidings with Preston Docks. This arch, which consisted of three rings as shown in Fig. 7, had to be removed, and the bridge re-constructed, in a new position, to make room for the widening of the lines south of Preston Station. The new bridge was first built and a temporary diversion made in the approach road, in order that there should be no stoppage of traffic to and from the goods yard. Six holes were "jumped" in the crown of the arch and nine holes in each haunch; the former being charged with 16 oz., and the latter with 28 oz. of tonite each. The crown shots exploded first, and crippled the arch, followed a few seconds later by the haunch shots, which again cracked the arch, but failed to bring it down. So badly cracked was the arch, however, that it was unsafe for the men to work on. A temporary staging was therefore erected, and the destruction of the arch completed from it, by hand.

The following prices paid for some of the materials on the works described, may be of interest:—

	£	s.	d.	
General excavation	0	1	6	per cube yd.
Excavation in trenches	0	3	0	„
Lime concrete	0	13	0	„
Cement concrete	0	17	0	„
Common brickwork	1	0	0	„
Blue brickwork	1	18	0	„
Extra for brickwork in cement .. .	0	6	0	„
Creosoted memel	0	3	6	per cube ft.
English oak	0	7	0	„
Steelwork	18	0	0	per ton.
Time fuses	0	0	3	per lin. yd.
Instantaneous fuses (subaqueous)	0	0	4	„
Igniters	0	0	3	each.
Tonite cartridges	0	1	4	per lb.
Gelignite cartridges	0	0	9	„

These prices are not quoted from any special schedule, but are an average of several contracts on which the author was engaged, and are typical for works in the north of England.

Altogether, whenever possible, a method such as is described above would appear to be the simplest, readiest, and cheapest plan, for adopting in similar circumstances; but could not of course be applied in a town or anywhere where an explosion was likely to cause damage. In such cases the arch must be taken down by hand. The easiest way to do this, if the distance between the limits of the minimum structure gauge, roughly 14 feet 3 inches above rail level, and the soffit of the arch is sufficient, is to erect skeleton centre ribs with laggings under the arch. When, however, the headway is not sufficient to allow for these skeleton ribs, a method such as was adopted some years ago during the opening out of the old Lime Street tunnel may be found useful. For this work a timber shield was used, formed of 16-inch by $\frac{5}{8}$-inch planks, bolted together every 12 inches with $\frac{1}{2}$-inch bolt, which were bent and wedged up under the arch of the tunnel till they roughly fitted the soffit, as shown in Fig. 8. Four thicknesses of planking being used when the arch was of brickwork and seven thicknesses when the arch was formed in rock, as shown in detail in Figs. 9, 10 and 11. Two wrought-iron cover plates were used to each rib and fastened with four $\frac{1}{2}$-inch bolts as shown in Fig. 12 and in the positions marked A.A. on Fig. 8.

In a short paper like this the author is unable to go into minute details, which of course vary slightly on each individual job.

In conclusion, the author would like to thank Mr. P. C. Stewart, A.M.Inst.C.E., for some of the information embodied in this paper.

DISCUSSION.

The President, in moving a vote of thanks to the author, said the meeting would agree that, although the paper was short, it was very interesting; he believed the subject had not been before the Society upon any previous occasion. The paper embodied many points of practical interest. There had recently been a rather similar case at the Crystal Palace. The old low level station was originally covered with a double span timber roof, and, it was decided that it ought to be taken down. The two inner ends of the trusses rested upon a central support running down the station, which consisted of a series of arches. In fact, the roof was on a viaduct or series of semi-circular arches forming part of the station.

After the roof was removed, the problem was to get this viaduct down. It was not destroyed in the manner described in the paper, but it was taken down by hand, which was a more difficult problem than was anticipated; because, after removing the upper portion and pulling out the spandrels, it was found necessary to bring a crane into operation and pull the arches down by main force. After considerable trouble, they fell down much in the way the author had described; and masses of brickwork lay on the ground, and had to be broken up with great trouble in order that they might be removed. In former days, brickwork was evidently more substantial than was supposed when its destruction had to take place.

Not very long ago, it was his fortune to see a fine chimney destroyed much as was described in the paper. The explosive in that case was cheddite, and the shots were fired by electricity, and not by means of fuses. He did not quite understand why, in the present case, electricity was not adopted, because this would have made it quite certain that every charge would go off at the time that was necessary. The result in the case of which he was speaking was very satisfactory. The chimney stack was undermined on three sides, and the two corner pillars of brickwork were left, so that it would fall sideways; and, when the charges went off, the chimney paused for a moment, and then lay quietly down on its side without a crack appearing till it reached the ground.

The author had given some prices which were always interesting, but he had not told them what it actually cost to take up the permanent way under the bridges and put it back again. He had given the price including the addition of the destruction of the bridge. Evidently, the question was a moot

point, some engineers preferring to cover up the permanent way, and others to pull it up and put it back.

He would like, before concluding, to call attention to two of the sketches. No. 1 was the arch which was increased in thickness towards the springing. He would like the author to tell them whether the extra ring of brickwork was put on in the way indicated in the sketch ; that is, whether there were simply more headers laid along the top, because that was not the ideal way of thickening an arch. The author had drawn attention in Fig. 2 to the alteration in shape of the slightly surcharged retaining wall. If that were a type of the retaining walls on that line, it was a wonder that any of them were standing. He gathered from the sketch that the line above the wall was on a curve, and he supposed that the retaining wall also was curving inwards. Perhaps the designer took an extra factor of safety from that. Possibly also, he was one of those engineers who were satisfied with too small a margin of safety, which, at any rate in the case of large cantilever bridges, recent events had proved to be inadvisable.

The Hon. H. W. Holmes à Court said that the paper was particularly interesting to him, for he was in charge of the works, described in the first part of the paper, for the contractor. The chief interest in the paper was the comparison between the two methods of obtaining the same object—the destruction of bridges. He might, perhaps, be allowed to hold a preference in favour of the method that the author had first described. The most striking comparison made was as to the amounts of explosives used—94 lb. in one case against 9 oz. in the other. He should like to know where the author got the information as to the 94 lb. If he got the information from one of the men on the works, the author had omitted to take off the usual 50 per cent. discount from what he had been told. The author stated that the charges in the crown were 16 oz., and those in the haunches 28 oz. The actual crown charges were about 6 to 8 oz., and the charges in the haunches from 12 to 16 oz., so that it was necessary to divide the 94 lb., roughly, by two. Then, one was a three-arch bridge, and the other was a one-arch bridge, so that they might again divide by three. Lastly, the bridge on the Eden Valley was destroyed by one charge on the crown only, and with nothing on the haunches, so that they might again divide by three. Although the experiment in the latter case was more or less a success in getting the arch down, the authors of it were, he supposed, duly grateful to Providence for their good luck, and he thought that they were more fortunate than they deserved to be. For instance, the author in his description of the three-arch bridge at Preston, stated that the

crown shots merely crippled the arch; that is to say, they cut the key out, which was what they were intended to do, and, in most cases, that was what happened; and it was not until the haunch shots were exploded that the main arch came down. In this case, judging from the photographs, the greater part of the arch fell, leaving the haunch overhanging. They had little less than five clear hours for the work, but they kept the train waiting for twenty minutes! That was explained entirely by the want of a second charge behind the haunch. It was pretty evident that, although there was not very much overhanging, it must have taken some little time to get it down, and during that time they could not start on the clearing away of the débris at the bottom. If they had had a second row of shots in the haunch, they would have got rid of all that delay, and they would not have committed the heinous crime of delaying the traffic for twenty minutes.

On the Eden Valley line they used gelignite. He could not claim much experience of gelignite, but he believed he was right in saying it was not so pleasant to deal with as tonite. With tonite they could do anything. They could smash it with a hammer, or they could even eat it, and, in fact, it was a very good cure for tooth-ache. He believed that they could do anything with it, if they did not detonate it. They very often got an unexploded charge left in the débris, and, so long as there was no detonator in the charge with tonite, they would be pretty safe. After the bridge was down, if they carefully traced every line of fuses in the ruins, whether burnt or not, and carefully withdrew any detonator that might have been left unexploded, there would not be any danger. With gelignite he believed that it was not so, and if they were going to stick a pick into gelignite, they might as well order their funerals beforehand.

The other bridge described by the author, that on the Docks branch at Preston, which was a failure, was very interesting, but it would have been more so, if the author had given any reason for the failure. He thought that he (Mr. à Court) could supply a reason, even if it were not the right one, and that was that the bridge was overcharged. He had had a somewhat similar experience on the Oxheys and Broughton widening, with the first bridge that he blew down. All the bridges along that line were very rotten indeed. In some of them the highest point was on either side of the crown instead of being at the crown, and the most rotten of all was the first that he started on. He believed the charges were rather more than he had stated. They were, perhaps, 10 to 12 oz. in the crown. At any rate, after the shot had been fired, he was horrified to see the arch still standing, and he found that the charges had simply blown out like a cork was

blown out of a bottle. They had blown the bricks out beneath them, and where every charge had been there was a hole, but the arch was still standing. Fortunately, it was not a serious matter, because the arch was so rotten that, by a few good charges, placed on the haunches and covered with clay, they finally shook the thing down. The lesson that they drew from that case was to reduce the charges in the crown. They never after that put more than 6 or 8 oz., and they put them as closely together as possible, 10 or 12 inches apart. Then, when they exploded, they could hardly fail to cut the key out of the crown. It was rather important to see that one haunch shot was exploded before the other. He had seen a bridge where the haunch shots exploded at the same time, with the result that the two portions of the arch simply fell forward and keyed themselves again, and there was considerable trouble in getting the arch down. The description of the blowing down of the Reading bridge pointed out a great mistake that he was responsible for, namely, putting charges in the pier of the bridge. It was the greatest possible mistake, because as the author stated, the charges simply lifted the upper half of the pier and dropped it again very nearly vertically. The consequence was that the men were engaged until long after dark in making the place sufficiently safe to satisfy the engineer in charge, and, as a matter of fact, the charge did not in the least help in pulling down the bridge—it only made the work dangerous.

The Eldon Street bridge, which was referred to, was very interesting as showing the impunity with which they could carry out such work, even in populous districts. Mr. Scott had mentioned a mill which, he said, was within 30 yards of the bridge. According to his (Mr. à Court's) recollection, the corner of the mill was only about 30 ft. from the nearest abutment of the bridge. At all events, it was very close. The chief danger was not so much the danger to the chimney, for the foundations of the chimney went well down below the formation of the railway, but it was the amount of glass that was likely to be broken, and he had a great deal of difficulty in persuading the resident engineer that the work was moderately safe, and also the manager of the mill, who was very anxious for his windows. The manager's opposition was withdrawn when he (Mr. à Court) undertook to make good any damage that might be done. Thirdly, there were the local authorities to be considered, but he merely asked the police to keep order during the pulling down of the bridge, and he did not tell them too much about how he proposed to pull the bridge down. The only precaution he took was to board up the large plate glass windows of the engine room which was close to the bridge. The other windows were so

numerous that it really would have cost more to board them up than it would to put them in again in case they were blown out. After the bridge was taken down, the manager of the mill told him that he had been unable to find a broken pane, and yet there was the mark of a flying brick on the boarding of the window which had been covered up, and there was another mark between two of the other windows, but not a pane of glass was broken. He, of course, took the usual precaution of bushing the arch and weighting it down, and so on. He could not claim any credit for the result, but he could claim a certain amount of good luck. They had a claim for damages in connection with the destruction of this bridge from an old lady who complained that her crockery was shaken off the dresser. This case showed the awful effects of high explosives, because the house where the accident happened was upwards of a mile away from the bridge, and the time at which the damage was done proved to be an hour *before* the explosion took place! Nevertheless, bricks sometimes blew a long way. He remembered that on one occasion three-quarters of a brick fell between him and a man to whom he was talking, and almost brushed their shoulders. It fell perpendicularly quite an appreciable time after the bridge was down. As a matter of interest, he measured the distance afterwards, and found that the place where the brick fell was over 7½ chains from the bridge. To what height the brick must have been thrown, it was appalling to think. In connection with the time occupied in blowing down a bridge, he had not any record of work on the Oxheys and Broughton widening; but he had had to deal with a bridge on the Great Western widening at Birmingham. The width of the bridge was 14 feet 8 inches. There were 13 holes on the crown of each arch, 13 inches apart, and the charge in each hole was 6 oz. of tonite. They had eleven holes on each haunch, each containing 16 oz. of tonite. He came to the conclusion after the explosion that they might have carried the crown shots a little farther than they did. They put them down to the top of the second inner ring, and they might, with advantage, have gone a little farther, and possibly they might have used a less charge than 6 oz. The charges in the haunches might have been rather less than 16 oz. He believed that 12 oz. would have been quite sufficient. They had possession of the line at thirty-three minutes past three. The word was given to fire at five minutes past four. The first road was clear at five minutes past five; that was in one hour. The second road was clear at thirty-five minutes past five, which was an hour and a half. He thought that that was almost "a record." The bridge was a very big three-arch bridge in a deep cutting, and he was not allowed to

remove any of the solid spandrels or the spandrel wall below the crown of the arch, so there was a lot of débris to be removed.

Mr. G. A. Goodwin said that, he being a mechanical engineer, the subject was somewhat out of his line of practice, but he should like to ask one or two questions which might be useful to those engineers who might be called to do such work as described without having had the experience the author had had. Could the author give them the cost of removing by hand a similar bridge to that which he had described, so that engineers might be able to form a comparison between removing by hand and removing by explosives? It was not quite clear to him whether the slow fuses were ignited by electricity. The author stated that the instantaneous fuses could not be of precisely the same length, but he thought that the author had made a little slip in that statement, as they could always make the fuses of the same length, if they simply coiled the fuses like a spiral spring. If that were done, they could make them of any required length.

With regard to the windows blowing out, he should like to ask whether there was any precaution taken in the way of opening of the windows, for he had always understood that windows broken by an explosion were broken, not by the wave of compressed air, but by the vacuum that was formed afterwards in the air outside. It was always found after an explosion that the windows had broken outwards. He remembered the dynamite explosion at the Board of Trade. In that case, the frames were blown outwards towards the explosion, instead of inwards. It would be interesting to know whether, in the case mentioned in the paper, the windows were left open, and whether that was the reason that the panes were not broken.

Mr. C. H. Shenton said that with regard to the chimney which was mentioned on page 65, a little more information would be very interesting. It seemed curious that it should be thought safe to destroy a bridge by means of explosives within 30 yards (or, as one of the former speakers had said, within 30 feet) of a chimney 100 feet high. Surely there must have been considerable risk of shaking the chimney, and perhaps of bringing it down. Was it really ordinary practice to carry out blasting operations so near to a chimney shaft?

The question of electrical firing the charges had already been raised in the discussion, and he supposed that there was some particular reason why the firing of the charges was not effected by electrical means rather than the old-fashioned method. He should have thought that there was much more certainty of getting the explosion, exactly at the time it was wanted, with electrical firing.

With regard to the prices mentioned in the paper, he thought that they were rather curious. Surely 3*s*. a yard for excavation was a very high price in ordinary shallow trenches. The paper did not say what depth the trenches were, or what was the nature of the ground. The matter wanted a little more explanation. With regard to common brickwork, 1*l*. a cubic yard sounded very low. He supposed that bricks must be very cheap in the district, but he had never known brickwork as cheap as that. For cement concrete, 17*s*. a yard was given, and he supposed that that was an average price, but one would expect that, in large masses, it would be cheaper.

Mr. Maurice Wilson asked for some information which he could not quite gather from the interesting photographs. On Plate II., on the right hand side, he saw a structure, and he was not quite sure whether it was meant for part of the mill referred to in the paper, or whether it was a wooden structure in connection with the works. Plates II. and III. appeared to be photographs both taken from the same spot, and the author, who was apparently present on the crown of the arch at the time, would know what the structure was, and could explain it.

With regard to the use of explosives, he could not say that he had had much to do with them for destructive purposes, but a good many years ago he was engaged on the Manchester Ship Canal with the contractor, where a good deal of tonite was used for the purpose of removing or loosening rocks, so that the steam navvies could operate. Tonite was employed, not because it was particularly good, but because it was not dynamite. He remembered dining one night with the contractor, the late Mr. T. A. Walker, and after dinner when the ladies had retired, one of the Cheshire magistrates and other local magnates said to Mr. Walker, "We hear you have tons and tons of dynamite stored up along the works. Is that true?" And Mr. Walker replied, "it is not true: it is tonite"; and the gentlemen who made the inquiry were perfectly satisfied. As long as the explosive was not called "dynamite," nobody minded at all. The rock in question, a shaly sort of formation, was loosened by means of tonite. Gangs of men were employed in "jumping" holes at intervals in the rock. They used long steel rods for the purpose, and they jumped holes to a depth of about 15 feet, and then the holes were enlarged by lowering tonite cartridges, about the size of one's finger, with a time fuse down into the hole, and setting them off. By these means, the holes were enlarged at the bottom, and then the next process was that a man brought a canister containing a certain amount of blasting powder and poured about a handful of the powder down the hole, with the idea of finding out whether any sparks had been left by the tonite cartridge.

It was thought that, if there was a spark left, it would set off the powder and that a puff of smoke would come up, and that then all would be right. He would then lower two tonite cartridges with time fuses down into the hole, and pour out of the canister about $\frac{1}{4}$ cwt. of the blasting powder into the recess, ramming the charge down with a wooden hammer, and, at the same time, his mate would go and fetch another canister which was some distance away, and pour that down. and this also would be rammed down. Then they would set fire to the fuse, and shout "Fire," and everybody would disappear. The charge would go off in the space of three or four minutes; but it always occurred to him that the whole proceeding was a little primitive and rash. It did not seem to follow that the pouring of a handful of powder into the hole would necessarily get rid of any sparks. There might be some sparks at the top of the hole. Four or five months after he (Mr. Wilson) got there, a navvy discovered some sparks. He had poured down about $\frac{1}{4}$ cwt. of powder, and began to ram it down, and his mate went to fetch another canister, when the half charge went off. A spark had been found. The immediate effect was that the wooden rammer, an instrument some 15 or 16 feet in length, was blown away, and it was afterwards found embedded in the mud of the River Mersey, a quarter of a mile away, and the ganger himself was very seriously damaged; in fact, he was blinded for some months.

Mr. H. Conradi said that his experience was in the direction of building bridges rather than of destroying them. He would like to ask whether, instead of destroying the arches in the way that was shown in the photograph, involving the great danger of the material falling and coming into contact with the trains, and also of destroying parts of the line through heavy weights falling upon it, it would not have been possible to lay very strong girders from the springing of the arch on one side to the springing of the arch on the other side, and thus form a very strong platform over the line, so that nothing could drop on the permanent way, while a sufficient headway was left for the locomotives to pass. They might then start from the parapet, take the same down, and gradually take down each part of the arch. He did not think that the expense of proceeding in this way would be much greater than the expense of blasting, and allowing the demolished blocks to drop down upon the permanent way, and having to stop the traffic.

Mr. R. W. A. Brewer asked whether the author had had any experience of firing shots down deep bore holes. This was a subject which had interested him (Mr. Brewer) very much lately. The other day a case came to him, and he decided that it would be advisable to fire a shot down a bore hole about

400 feet deep, but there was great difficulty in getting the sanction to do it, because people were very nervous about the firing of a shot anywhere. As he knew that the hole was 400 feet deep, he did not, from his own point of view, see that there would be any risk to any external property from firing a shot at that depth. If any gentleman present had done any work of this kind, and could give information on the subject, it would be very interesting, because cases of the kind occurred from time to time. The other day he spoke to a man who had fired some shots down a bore hole in Ireland, and what took place was this: he first fired a 20-lb. shot of bellite at a depth of about 400 feet, and this shot of 20 lb. shattered the rock and filled the hole up to a depth of 100 feet from the original bottom, so that the shattered rock was 300 feet from the surface, but the result aimed at was not attained. Therefore, another cartridge was let down to a depth of 300 feet and fired, also without result. As the explosion did not seem to dislodge anything, two 40-lb. cartridges were lowered. The hole was bored at the bottom of a sunk well, 50 feet deep, and tubes had been put into the well, so that anything which happened down the bore hole should not interfere with the well water, as this had to be kept in order. When the two 40-lb. cartridges were fired, they filled up the sunk hole, and the bore hole was lost. The explosion removed some rock, but he did not think that it did any damage to adjacent property.

In replying to the questions which had been asked, **Mr. Scott** said he would answer the President first. The reasons why the charges were not exploded by electricity were two-fold, first, it would have been more expensive, and secondly, the connections would be very hard to make; the explosion of the crown shots in all probability breaking the connections of the haunch shots. With regard to the cost of taking out the permanent way, and relaying, it was not very much. A total length of about 60 yards was removed, and, allowing 1*s.* per yard for taking out, and 1*s.* 6*d.* per yard for relaying, the cost would be about 7*l.* 10*s.* Had there been any junctions removed, as was the case on the Eden valley work, the cost would have been: taking out 60 yards of plain road at 1*s.* per yard, taking out 60 yards of junction at 1*s.* 6*d.* per yard, relaying 60 yards plain road at 1*s.* 6*d.*, and relaying 60 yards of junction at 2*s.* 6*d.* per yard—total 19*l.* 10*s.* It will thus be seen that "junction" work adds heavily to the expenses. It must also be remembered that these prices are for labour only and do not include any new materials. Perhaps a few prices for materials may be of interest:—

	£	s.	d.	
Steel rails (90 lb. per yard)	5	7	6	per ton.
" fishplates (30 lb. per pair)	6	0	0	"
Cast-iron chairs (45 lb. each)	3	5	0	"
Sleepers (9 feet by 10 inches by 5 inches)	0	4	10	each.
Points (complete)	10	10	0	per set.
V-crossings (complete)	9	10	0	"
Diamond crossings (complete)	12	0	0	"
Granite ballast	0	4	0	per cubic yard.

When using the word "masonry" he was referring to stone-work only. Fig. 1 was quite correct, and was a copy of a sketch made at the time the work was in progress. It must be remembered that this bridge was built over fifty years ago, when engineers were not so careful as they are now, and the bridges did not carry such heavy loads. In regard to sketch 2, he (the author) had given the worst part of the wall, which was raking down from its full height to a fence wall only 4 feet 6 inches high. The cause of the wall coming over, as it had done, was the extra weight of the locomotives, and the increased speed. At this part of the line most of the locomotives used are of the 4·6·0 simple type, weighing, with their tender, 103 tons, and travelling at from 70 to 80 miles an hour.

With regard to Mr. à Court's questions, he (the author) quite agreed that tonite was much safer to use than gelignite. Some of the gentlemen present may have seen an account of a recent accident on the Dearne Valley Railway. A gelignite cartridge fell amongst the débris unexploded, one of the navvies happened to strike it with a pick, and he with two of his mates were killed by the following explosion. Had tonite been used this would probably not have occurred. The weights of the charges used were obtained from some notes made at the time, and corroborated a few weeks ago, by one of the assistant engineers, who was also on the works at the time. If Mr. à Court thinks the charges are excessive, he (the author) would like to call his attention to a very similar bridge at Melton. In that case 138 lb. of tonite were used, the charges being made up as follows:—

174 No.	$1\frac{1}{4}$ inch	4 oz.	tonite	cartridges.
94 "	"	6 "	"	"
80 "	"	12 "	"	"

The reason for the failure in blowing down the arch over the Ribble branch was that the holes were overcharged, and the result was that the explosives simply made holes in the arch, and blew the stone-facing vousoirs off.

As to the distance of the chimney at Eldon Street mill from the bridge he had never actually measured the distance, but, unless his memory was at fault, the near corner of the mill was

about 30 feet from the bridge, but the chimney was at the further corner of the mill, and quite 30 yards away. With regard to the time taken to bring down the bridge at Birmingham, and clear away the débris, he would like to say, that, although very quick, it was not quite a record; two hours (3.33 to 5.35 p.m.) being required for the work, whereas a similar bridge, near Sonning, also on the G.W.R., had been removed in 1¾ hour (12.0 noon to 1.45 p.m.).

In answer to Mr. Goodwin's first question, the cost of removing the arch by hand would have been considerably more. Taking the 200*l.* as our basis, we should have to deduct 24*l.* for the cost of the explosives, jumping holes, clearing away débris, etc.; which would leave 176*l.* for pulling down the parapets, spandrels, abutments, etc. To this must be added the cost of removing the arch, erecting and removing centreing, flagmen's time, etc., which amount would be about 98*l.*; giving a total of 274*l.*, or 74*l.* in excess of the method described in the paper. All windows near the bridge should be left open, both top and bottom sashes.

With regard to Mr. Shenton's remarks as to the safety of blasting within 30 yards of a chimney shaft, of course, a lot depended on the condition of the chimney, and the foundation on which it stood; but, granted that the stack was in good repair, and stood on a solid foundation, he thought the blasting operations could safely be carried out. He might, however, mention that during the destruction of a bridge at Goring, near Oxford, the roof of a cottage 50 yards away had been damaged by falling brickwork. With regard to the prices, he would ask Mr. Shenton to notice that these prices are averages of a number of contracts. He had once paid 3*s.* per yard for excavation, and in that case the price included timbering, to a depth of about 15 feet, and the "muck" was wet sand, necessitating a lot of pumping. Brickwork at 20*s.* per yard was a very fair price for the north, and he had got it done as low as 19*s.* per yard.

The building shown on Plate II., mentioned by Mr. Maurice Wilson, was an old signal cabin, which was removed some 200 yards southwards, before the bridge was destroyed. Mr. Wilson's description of the blasting on the Manchester Ship Canal certainly seemed a little primitive, but this work was probably done before such things as the Employers' Liability Act came into force.

In regard to Mr. Conradi's question, he was not quite clear what Mr. Conradi meant. If he erected a girder platform at springing level, he would, in most cases, block the lines for some days, as there was not sufficient headway. Even if there were, he would also have to erect centreing, or else as soon as he

cut the arch, it and his workmen would fall together on to the platform. He would also have to be careful as to his side clearance; the vertical supports to the platform must be at least 4 feet 6 inches from the nearest rail. In any case the cost, as already shown, would be greater than the method described in the paper.

With regard to Mr. Brewer's question, he was very sorry he could give no information, as he had had no experience in deep bore holes.

In conclusion he would like to thank the gentlemen who were present for the kind manner in which they had listened to his paper. He would also like to thank Mr. J. C. Farmer for the use of some of the photographs shown during the evening.

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PLATE I.

THE DESTRUCTION OF ARCH BRIDGES.

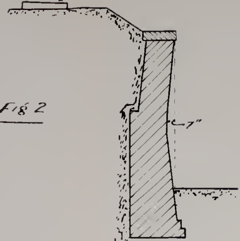
H. C. D. SCOTT.

Fig. 1.

ELEVATION OF ARCH

Scale ¼ inch to 1 foot

Fig 2



Scale ⅛ inch to 1 foot

Fig: 3

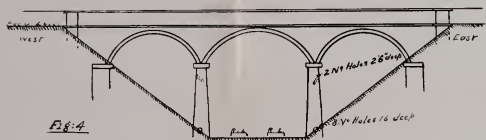


Fig:4

Scale 41.66 feet to 1 inch

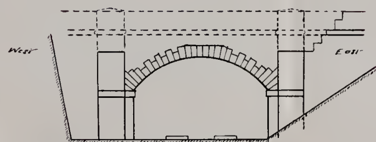
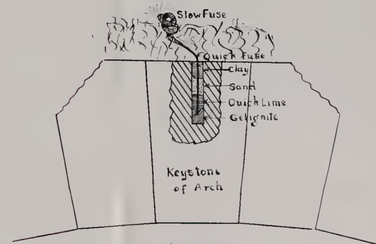


Fig 5

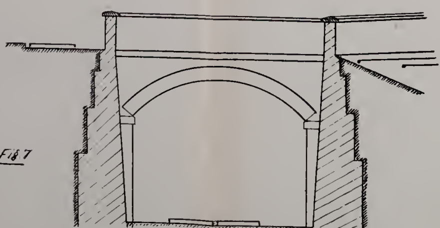
Scale ⅛ inch to 1 foot



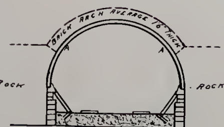
Scale:- 1in = 1 ft.

Fig: 6

Fig 7



Scale ⅛ inch to 1 foot



LIME STREET TUNNEL

Fig 8

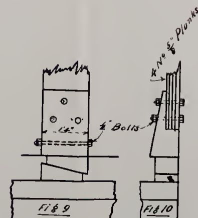


Fig 9

Fig 10

DETAIL of foot of Shield for BRICK ROOF

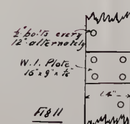


Fig 11

DETAIL of outer joint cover at A

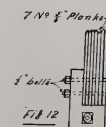
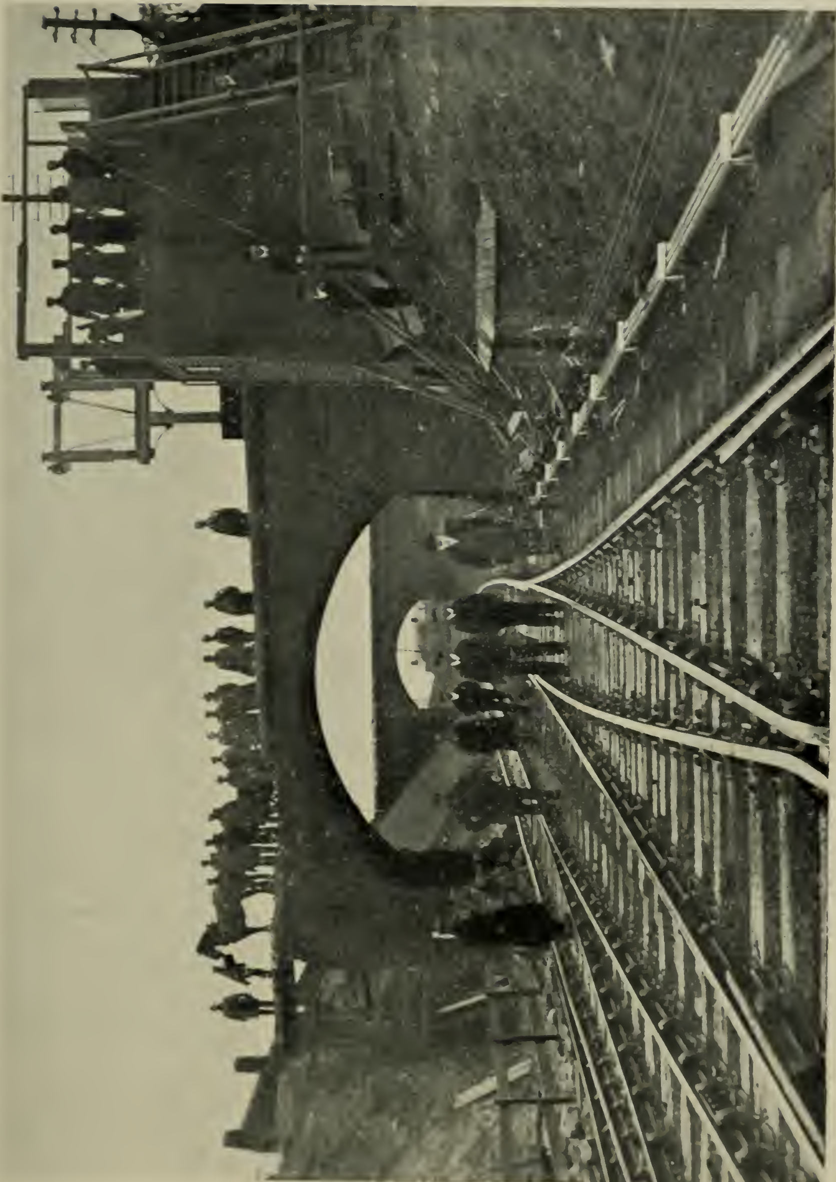


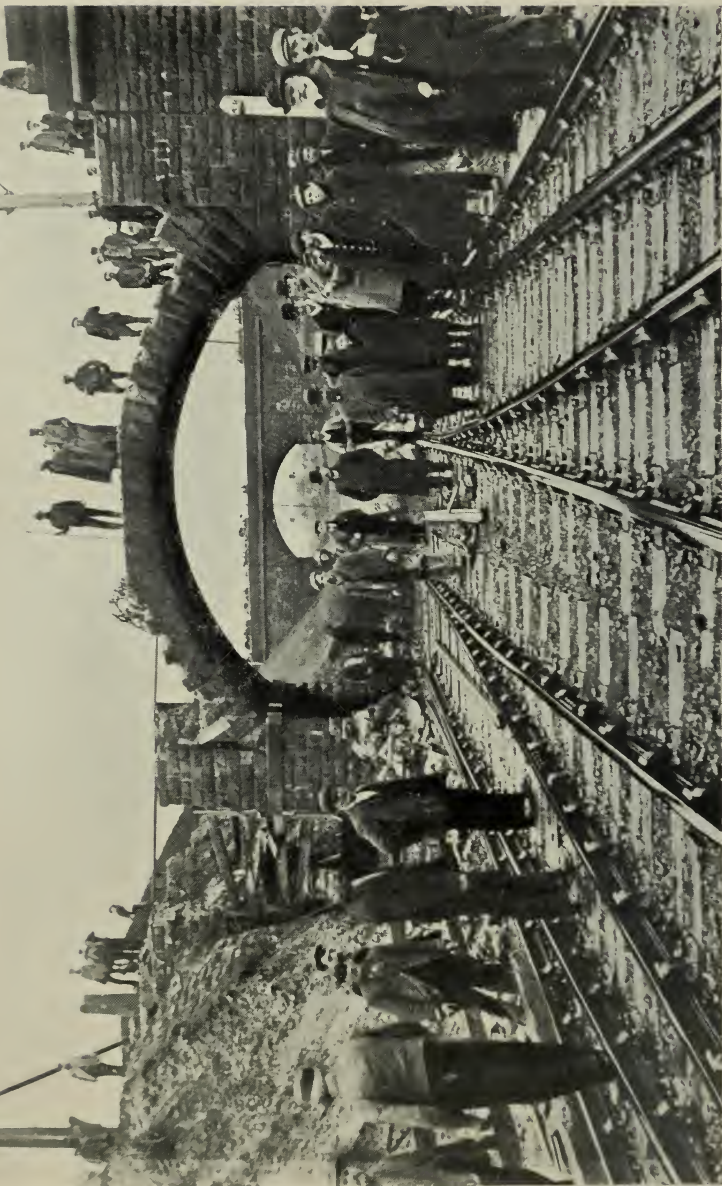
Fig 12

DETAIL of foot of Shield for ROCK ROOF

PLATE II.

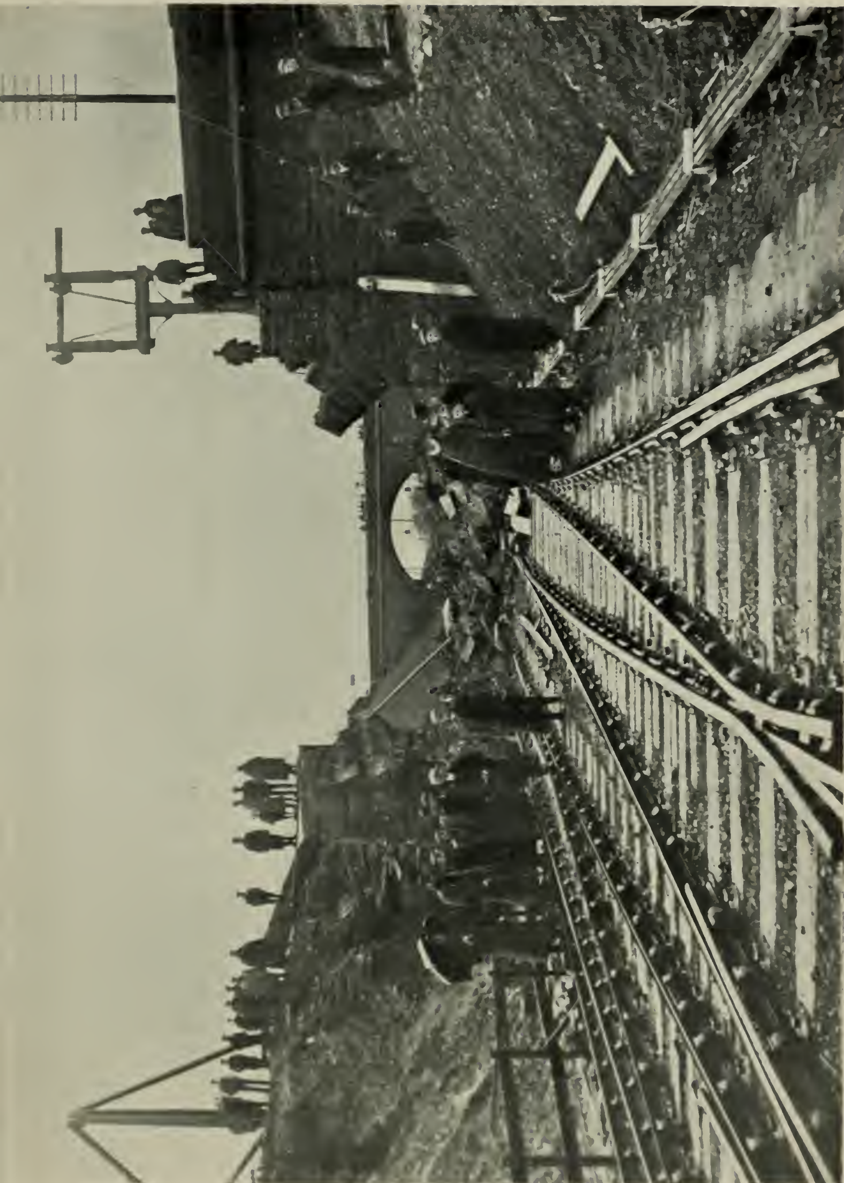


Bridge Destroyed at Eden Valley Junction.



Bridge Destroyed at Eden Valley Junction.

PLATE IV.



Bridge Destroyed at Eden Valley Junction.

PLATE V.

Bridge Destroyed at Eden Valley Junction.

May 4, 1908.

JOSEPH WILLIAM WILSON, President, in the Chair.

THE DESIGN, AND THE WASTE AND WEAR OF WHEEL TEETH.

By Prof. Robert H. Smith, A.M.I.C.E., M.I.M.E., M.I.E.E.

Throughout the paper the following meanings are attached to letter-symbols:—

When two wheels in gear with each other are considered simultaneously, N means the number of teeth in the wheel whose tooth-face is in contact at the instant considered, and n the number of teeth in the other wheel whose tooth-flank is in contact.

P = peripheral pitch.

The suffix o particularises quantities close to the pitch-point or at the shoulder of the tooth.

τ = distance of touching-point from pitch-point.

i = angle between τ and a normal to the line joining the centres of the wheels.

i_o = value of i close to pitch-point.

R = radius of curvature of the outline of the face of a tooth.

r = ditto of the flank of a tooth.

R_o and r_o = values of R and r at the shoulder.

D = normal distance of centre of curvature of the outline of the face from the line drawn through the pitch-point at right angles to the line of centres.

d = ditto of centre of curvature of outline of the flank.

D_o and d_o = values of D and d for R_o and r_o at the shoulder.

f = coefficient of friction between touching face and flank.

γ' = relative angular velocity between two wheels in gear = sum of their angular velocities relative to the frame.

The object of the present paper is to explain a new shape for the teeth of spur, bevel, and helical-toothed wheels, whereby the loss of power by friction and the abrasive wear of the faces and flanks of the teeth in the transmission of power by toothed

gear are diminished to the lowest possible limits consistently with the maintenance of uniform velocity-ratio and of interchangeability between all wheels of the same set developed from the same base, and whereby greater average strength of teeth is also secured.

With the great extension in recent years of the use of high-speed gearing, the good and bad running qualities of toothed wheels have become of even greater importance than in the days when mill-gearing was their chief and almost only application. In the ordinary treatises, as also in common practice, the only two considerations, on which is based the design of wheel-teeth, are strength and strict uniformity of velocity-ratio. In 1852 Edward Sang published a book, "The Teeth of Wheels," which is still the only scientific investigation of the numerous desirable qualities that may be possessed by such teeth. Mr. Sang was a practical mechanic and a skilled mathematician, but his methods of investigation were so laborious, and his style of exposition so pedantic, that his work remains to this day practically unknown.

The general line followed by his theory was as follows :—

The normal to the touching surfaces of two wheel-teeth in gear with each other intersects the line of centres in what is called the "pitch-point," and this point divides the distance between the centres in the inverse ratio of the angular velocities of the wheels, provided these keep in close contact. The pitch-point must therefore be kept constant if the velocity-ratio is to remain constant.

Under this condition of constant velocity-ratio, for a given shape of tooth on one wheel, this shape and the fixed pitch-point completely specify the "path of contact," that is, the curve along which contact between two gearing wheels travels as the wheels revolve. Conversely, the pitch-point and the path of contact together determine the shape of the tooth for any wheel with given centre. If the same contact-path be used for the designs of a set of wheels of various sizes, then any two of these will gear with each other so as to maintain constantly the same velocity-ratio. Edward Sang investigated the effects of differently shaped contact-paths in giving the wheels designed from them various degrees of the following eight good and bad qualities: (1) Maximum and minimum obliquity of driving thrust; (2) number of effective driving contacts simultaneously in action; (3) smallest number of teeth of pinion giving practicable shape; (4) undercutting of the flank in small pinions; (5) sharpness of curvature in tooth outline near the pitch-circle; (6) work spent in frictional sliding; (7) abrasion of contact surfaces; and (8) liability to wear out of true shape.

He investigated the properties of five kinds of contact-path;

among them the straight line giving the well-known "involute" teeth, and the circular-arc-path, giving the other common form "epicycloidal" teeth. The contact-path chosen by him as best he called the "modified hour-glass," which is a mixture of two curves, which he calls the "hour-glass" and the "kemend." This admits of pinions of a minimum number of 14 teeth with two effective driving contacts continuously in action, without undercutting, and without sharp curvature at the pitch-line. Cycloidal teeth admit of 11-toothed pinions, but they are undercut and have sharp pitch-line curvature. It is mechanically impossible to cut cycloidal teeth to exactly true form. Involute teeth have many good qualities, that which makes them popular in many shops being their property of still gearing correctly when the centre-distance between a pair is inexact; but they labour under the disadvantage of excessive obliquity of driving thrust.

Modern experience has led to item (2) being considered of little or no importance. To ensure two contacts being continuously maintained, it is obviously necessary to use long teeth, and the breaking strength of a tooth measured by the thrust is, for a given thickness, inversely as its length. The idea that the total driving thrust is nearly equally divided between the two pairs of teeth in contact, is delusive, and it is fatally unsafe to design the strength of the tooth on the supposition that it is so divided. Thus the use of long teeth leads to more frequent breakages, and the very worst results of the two-contact design is that a wheel may still be run after a tooth has been broken. This proves, of course, that the dimensions have really been designed of sufficient strength for single contact. But, as for single contact they might have been made shorter, and so stronger, it is clear that the design is futile and uneconomic.

It is also to be noted that if the design results geometrically in double-contaot during say three-quarters of the passage, and single-contact only during the remaining one-quarter, then the tooth must be made strong enough for single-contact, and the double-contact during a portion of the time is useless, so far as strength is concerned, even with dead accurate shaping and fitting. It might possibly have some advantage in respect of abrasion and wear, although probably none in respect of frictional waste of driving power.

Nevertheless, each following pair of teeth must be well in gear before the preceding pair has parted contact. So that the geometrical design, in respect of tooth-length, must provide for what may be termed $1\frac{1}{4}$-contact, that is the angular length of the actual path of contact must be from $1\frac{1}{6}$ to $1\frac{1}{3}$ of the length needed for single contact. This proportion increases with the number of teeth in the gearing wheels. If it be made $1\frac{1}{10}$ or

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$1\frac{1}{8}$ for two smallest-size pinions gearing with each other—a combination never used for the transmission of large powers—it will be from $1 \cdot 3$ to $1 \cdot 4$ for two large wheels in gear.

During approach to the pitch-point the flank of the driving tooth bears against the face of the driven tooth; and after the pitch-point is passed the face of the driving tooth bears upon the flank of the driven tooth. The contact, therefore, begins where the path of contact cuts the circle of the tips of the teeth of the driven wheel; and it ends where the contact-path cuts the circle of the tips of the teeth of the driving wheel. For obvious reasons, except in special work, the two parts of the contact-path on opposite sides of the pitch-point are the exact reverse of each other. Thus, if the driver be the smaller of the two wheels, as in case of a pinion driving a wheel, the contact during approach to the pitch-point extends through a longer arc and a longer contact-path than does the contact during recess from the pitch-point.

If the motion be reversed, the wheel now driving the pinion, the same contact-path will be traced out backwards; but if, with the reversed motion, the pinion still drives the wheel, then the contact-path is changed to another similar curve of reversed inclination to the pitch-circle tangent at pitch-point. This is because the bearing between the teeth is now upon the other sides of the teeth. If, as is usual, the two sides of the teeth are symmetrically reverse, then the forward and backward branches of the contact-path are also reversely identical. Unless reverse-driving is sometimes required, there is no good reason for having the back and front of the tooth reverse identical curves; but for convenience in stocking wheel-castings and patterns, wheel-teeth are nearly always made so—at the sacrifice of some strength and other good qualities.

Teeth so short as $\frac{1}{5}$ of the pitch above the pitch circle have long been in successful use in Lancashire. When the wheels are of large size, this gives contact on each side of the pitch-point through about $\frac{6}{10}$ of the pitch, but the face-contact of a pinion lasts barely over $\frac{1}{2}$ of the pitch, which appears very undesirable.

In the design of tooth recommended in this paper, the height is made $\frac{1}{4}$ pitch above pitch-circle, and the shape of contact-path adopted yields, with this height of tooth, a $\frac{2}{3}$ pitch-contact on either side of the pitch in the rack and very large wheels, and $0 \cdot 56$ pitch on the face of a pinion of 12 teeth. Two small pinions would gear with each other through $2 \times 0 \cdot 56 = 1 \cdot 12$ of the pitch; while a small pinion would gear with the straight rack or any very large wheel through $0 \cdot 56 + 0 \cdot 67 = 1 \cdot 23$ of the pitch. The length of the face contact rises towards the ultimate $\frac{2}{3}$ very

rapidly with the number of teeth, so that with only 30 teeth it is already over 0·64 of the pitch.

An important advantage of short teeth is the greater accuracy of form attained in practical manufacture. Both in the drawing office and in the machine shop, the theoretically correct shape is invariably approximated to by means designed to lessen labour. The errors due to such approximations increase in geometrical ratio with the length of the tooth.

Another important advantage is an increased frictional efficiency. It will be shown that the velocity of sliding of tooth upon tooth is $\tau \gamma'$, where τ is the instantaneous distance of the touching point from the pitch-point, and γ' is the relative angular velocity between the wheels—that is, the sum of their angular velocities relative to the frame. Here γ' is unaffected by the shape of the contact-path, and τ is zero when the touching point passes the pitch-point, while its extreme values at beginning and at end of contact depend almost solely upon the general inclination of the contact-path, the length of the teeth and the number of the teeth. In Appendix VI. it is demonstrated that the ratio of

$$\frac{\text{frictional waste work}}{\text{effective driving work}} \propto f \frac{n + N}{n N},$$

where f is the coefficient of rubbing friction, and n and N are the numbers of teeth in the two wheels in gear. The proportion depends on the shape of the path of contact, the numerical factor being 1·06 for the design developed in this paper, and about 1·8 with involute teeth. In order to make this factor low, the path of contact should at its passage of the pitch-point make only a small angle with the tangent to the pitch-circles: this in order to avoid much obliquity of acting thrust. But if this angle were made zero, the radius of curvature of the tooth outline close to pitch-point must be zero for maintenance of uniform velocity-ratio. (See Appendices II. and III.) This is true whatever shape of contact-path be adopted. It means a theoretically sharp knife-edge shoulder to the tooth, and this is not only impossible of manufacture, but would also produce extravagantly rapid wear. The choice of obliquity of contact-path at its crossing of the line of centres is, therefore, a matter of compromise, into which also enters the consideration of undercutting in the teeth of small pinions. In the design of this paper the angle $4° \, 35\frac{1}{3}'$, whose sine = ·08, is chosen. If the thickness of the tooth of a 12-teeth pinion were made parallel inside the pitch-circle, the sides of the gap between the teeth would converge at an angle $\frac{360}{12} = 30°$; if, on the other

hand the flanks were made radial, the gap angle would be about 16°, the tooth thickness being about $\frac{7}{8}$ the gap width. With the above angle $4° 35\frac{1}{3}'$, the gap-flanks converge at about 25° in the 12-tooth pinion, and in the straight rack at pitch-line the convergence is $9° 10\frac{2}{3}'$. For any number of teeth N the convergence is about $\frac{192°}{N} + 9° 10\frac{2}{3}'$. It is not recommended that pinions of 12 teeth should be used except when unavoidable. 12 is here taken as the minimum, which still avoids the development of actual faultiness ; but pinions of 14 and 15 teeth have materially better form and smoother action, greater strength, and better frictional efficiency. With a coefficient of friction of $\frac{1}{20}$ and a gear ratio of 4, the above formula for the frictional inefficiency gives :

No. of teeth in pinion =	12	13	14	15
Frictional inefficiency =	0·00534	0·00492	0·00457	0·00426

For a given sum of teeth in the two gearing wheels, this inefficiency reaches its minimum when the wheels are equal. Its value is then $1 \cdot 06 f \frac{2}{N} = 2 \cdot 12 \frac{f}{N}$. To illustrate its variation with the gear ratio, take $n + N = 120$, and $f = \frac{1}{20}$. The following are the results :—

$n =$	12	18	24	36	48	60
$N =$	108	102	96	84	72	60
Frictional inefficiency =	0·00474	0·00335	0·00267	0·00203	0·00178	0·00171.

For any given gear ratio, this inefficiency is inversely proportional to the number of teeth in either wheel

The effective coefficient of friction largely depends upon the retention of the lubricant between the bearing surfaces, and this again is greatly influenced by the closeness or openness of fit between the two. During approach to the pitch-point the flank of the driver presses against the face of the driven tooth, and during recess the face of the driver bears against the flank of the driven tooth. Thus the gear is always between a face and a flank. The faces are necessarily convex. To obtain the greatest practicable closeness of fit, the flanks should be concave. The flanks become more and more concave as the number of teeth becomes greater, and therefore, it is the flank of the smallest pinion that should be most carefully considered. The design here presented contents itself with avoidance of convexity in the flank of the

pinion of 12 teeth, which it makes straight, that is, neither convex nor concave.

The mathematical measure of the closeness of fit between two curved surfaces is one-half the difference of the curvatures, or $\frac{1}{2}\left(\frac{1}{R} - \frac{1}{r}\right)$ where R and r are the radii of the convex and concave surfaces respectively. This equals the distance apart of the two surfaces at unit distance to either side of the mathematical touching point, the unit of length being chosen small. The clear space at any small distance from the touching point, equals $\frac{1}{2}\left(\frac{1}{R} - \frac{1}{r}\right)$ multiplied by the square of this arc length.

The fit is least close when the two shoulders touch as they pass the pitch-point. When the numbers of teeth are N and n, the design described in this paper makes the shoulder closeness of fit equal to $\frac{14 \cdot 7}{\text{pitch}} \cdot \frac{N + n}{(N + 3)(n - 3)}$. Thus for 20 and 80 teeth with 2″ pitch, the clear space at $\frac{1}{10}''$ to either side of the touching point is 0·00415″ when the flank of the pinion touches the face of the wheel and 0·00521″ when the pinion face touches the wheel flank close to the shoulders.

After contact has passed a very small distance from the shoulder, the fit becomes very much closer and remains constant. For example, when the face of the 20 pinion touches the flank of the 80 wheel, the clear space at $\frac{1}{10}''$ on either side of the touching point is 0·0016″, and it is 0·0027″ when the 80 wheel face touches the flank of the 20 pinion.

It is important to note that this closeness of fit is inversely proportionate to the pitch. Large pitch is therefore desirable not only on account of strength, but also for the sake of frictional efficiency in power transmission and for that of minimisation of abrasive wear.

Closeness of fit, is, in the opinion of the writer, of even greater importance from the point of view of abrasion and wear. With unguent of given "strength" or viscosity, the bearing thrust will be distributed over a larger area in a proportion to this closeness of fit, and the intensity of bearing pressure per square inch is proportionately reduced. There can be hardly any doubt that the abrasive wear increases rapidly with this intensity of pressure. The system of tooth-gearing of this paper has been designed more in view of minimum wear than for any other object. In the case of pinion gearing, the closeness of fit is practically uniform throughout the whole of the passage, and in wheel gearing there is very little variation.

This desideratum of closeness of fit really includes the avoidance of sharp curvature at the shoulders. Every mechanical engineer recognises instinctively that sharp curvature in a surface that is to rub over another under heavy pressure is most objectionable. With any approach to even a very blunt knife-edge curvature, indentation of the bearing surface occurs, and this both largely increases the resistance to dragging the one surface over the other, and also much diminishes the load at which actual crushing of the material begins, such crushing being the preliminary to abrasive wear. Again, curvature not nearly so sharp as to produce these results is yet sufficient to ensure the flatter surface being scraped bare of oil or grease. It is not generally recognised that in both cycloidal and involute wheel-teeth, the theoretical curvature at the shoulders is infinitely sharp. Fortunately it is impossible to manufacture teeth to this theoretically true shape at the shoulders, and this practical impossibility is the only reason why these teeth do not crush and suffer extravagantly rapid abrasive wear at their shoulders.

In the design here advocated, the radius of curvature at the shoulders is made as large as is compatible with other good qualities already mentioned. In order to reduce the obliquity of driving thrust within moderate limits, and at the same time secure $1\frac{1}{3}$-pitch rack-contact with a height of tooth above pitch-line of $\frac{1}{4}$ the pitch, the path of contact is made to cut through the pitch-point at a sine-inclination of $0 \cdot 08$ to the tangent, or $4°\ 35\frac{1}{3}'$, and the ratio of this angle to $\frac{1}{4}$ of $30° = 7\frac{1}{2}°$ is $0 \cdot 612$, where $30°$ is the pitch angle of the 12-teeth pinion, the theoretical minimum number of teeth. This makes the theoretical radius of curvature of the face of the 12-teeth pinion close to pitch-point $\frac{0 \cdot 612}{8} = 0 \cdot 0765 \times$ pitch, and that of the rack $\frac{0 \cdot 612}{4} = 0 \cdot 153 \times$ pitch. For any number of teeth N, it becomes $R_0 = 0 \cdot 153 \frac{N}{N+12} \times$ pitch.

The similar radius of curvature for the flank close to pitch point is infinitely long for a 12-teeth pinion, is $0 \cdot 153 \frac{N}{N-12} \times$ pitch for any number of teeth N, and for the rack it becomes $0 \cdot 153 \times$ pitch, or equal to the face-radius.

As the touching point recedes from the pitch-point, the obliquity of the driving thrust to the pitch-point-tangent increases at a uniform rate with the angle rotated by the wheel

up to a final limit of $5 \times 0 \cdot 08 = 0 \cdot 4$ by sine-measurement at the end of the full rack-contact, although it does not reach so far in the pinion contact. During the first motion from pitch-point while this increases from $0 \cdot 08$ to $2 \times 0 \cdot 08 = 0 \cdot 16$, the theoretical face radius of curvature increases from the above values to four times as much in the 12-teeth pinion and to three times as much in the rack, that is, to $0 \cdot 306 \times$ pitch in the pinion, and to $0 \cdot 459 \times$ pitch in the rack. For any number of teeth it increases to $0 \cdot 153 \frac{3\,N + 12}{N + 12}$

During the same small motion the flank radius of curvature in the 12-teeth pinion remains infinity, the whole flank being straight, while in the rack it increases to thrice its length at the pitch-point, the flank and face in the rack being the exact reverse of each other. For any number of teeth N it increases from the already given value to $0 \cdot 153 \frac{3\,N - 12}{N - 12} \times$ pitch.

This arc of the tooth-line is very short in all cases, as is seen in Figs. 10 to 13, where the outlines of a series of teeth are drawn for 4-inch pitch and where this arc is marked off in each case above and below the pitch-circle. An elaborate trigonometrical calculation shows that a single arc of radius equal to the arithmetical mean of the two radii at the beginning and end of this element gives the true outline without any measurable error, that is, on a 4-inch pitch the error would nowhere be so much as 1 mil. These mean radii are therefore used in this design for the two small shoulder arcs on face and flank. They are :—

	Shoulder Face Radius.	Shoulder Flank Radius.
12-teeth pinion . .	$0 \cdot 191$ P . .	Straight $= \infty$.
N teeth	$0 \cdot 306 \frac{N + 3}{N + 12}$ P . .	$0 \cdot 306 \frac{N - 3}{N - 12}$ P.
Rack	$0 \cdot 306$ P . .	$0 \cdot 306$ P.

where P means the pitch. The normal distance of the centre of either of these arcs from the pitch circle tangent at pitch-point is $0 \cdot 08$ of its radius.

From the ends of these short arcs to the extreme limits of the path of contact, while the sine of the obliquity of thrust increases from $0 \cdot 16$ to $5 \times 0 \cdot 08$, and while the distance from pitch-point to point of contact lengthens from $0 \cdot 1525$ P to $0 \cdot 6108$ P or fourfold, the theoretical radius of curvature of the

face of the 12-teeth pinion increases from $0 \cdot 306$ P to $0 \cdot 995$ P, or in the ratio $3\frac{1}{4}$, while that of the rack increases from $0 \cdot 459$ P to $1 \cdot 377$ P, or in the ratio of 3. At the same time that of the flank of the 12-teeth pinion remains infinite, and that of the flank of the rack increases in the ratio 3; while that of any wheel of N teeth is increased in the ratio $\frac{3(N - 5\frac{1}{3})}{N - 4}$, which is less than 3. This arc being much longer than the other, sufficient accuracy is not yielded by using a radius equal to the arithmetical means of the theoretical radii at its two ends. Nor is any satisfactory approximation given by using a radius of curvature equal to the mean radius taken with respect to the angular motion of the wheel, because for equal small rotations of the wheel the corresponding lengths of tooth-outline are very different.

The only satisfactory simple approximation to the true shape is an arc of a circle which will run tangentially into the short shoulder arc found above, and which will also give to the outline at the point and at the root of the tooth (1) its true position and (2) its true direction. The problem of finding two such arcs is a tedious trigonometrical calculation. The numerical values of the radii and the positions of the arc-centres are given for a series of wheel sizes in the diagram, Fig. 9, in which are plotted curves giving all the four radii of the two shoulder arcs and the two completing arcs, along with the positions of their centres. These positions of the centres are best defined by their normal distances from the tangent at the pitch-point. Although the formulæ given are approximations to the exact results required for absolute uniformity of velocity-ratio, still the approximations are extremely close, considerably closer than is practically needed or is practically possible in the best machine gear-cutting. On the face of the 20-tooth pinion to 10″ peripheral pitch the error lying inside the true curve $= 0 \cdot 02'' = 0 \cdot 002 \times$ pitch. In respect of uniformity of velocity-ratio, it must be remembered that exact equality of pitch from tooth to tooth is more important than exact proper shape of outline. It is in the change-over from contact between one pair of teeth to contact between the next pair that shock and noise occur, and this is due to inexactness in the equal pitching. Deviation from true outline of tooth profile produces some acceleration of one or the other wheel, and a corresponding strain and stress proportionate to the masses accelerated; but this acceleration is never sudden in the same sense, or the same order of magnitude, as is the spring into contact of the following pair of teeth when that of the preceding pair ceases and the spacing is inexact.

This latter produces hammer-blows, while the former produces only quiet stressing. Exact pitching is of prime importance, while the good shaping of the tooth is of more importance for the sake of frictional efficiency and absence of abrasive wear than for uniformity of velocity. From this latter point of view single-arc faces and flanks are very desirable, because they maintain one uniform closeness of fit during the whole period of approach to the pitch-point, and again during the whole period of recess from pitch-point. This distributes the wear evenly over the whole face and the whole flank, so far as this wear depends on closeness of fit. It is impossible to carry down the same arcs to the pitch-line, because they would meet in a sharp edge, and moreover neither of them would give the crossing of the pitch-circle at the correct inclination. Thus the short connecting shoulder arcs are essential for smooth running and for minimisation of wear, and four arcs constitute the simplest possible design for really satisfactory results. The design here recommended makes the radii of curvature at the shoulders as large, and the closeness of fit throughout as close, as is possible under the two conditions, (1) uniform velocity-ratio in all gears, and (2) interchangeability of all pairs of wheels that can be made up from a complete set ranging from the 12-teeth pinion to the straight rack. The writer attributes very little importance to ease of draughtmanship, because an hour's extra labour in the drawing-office of the employment of a higher grade of draughting skill is as nothing compared to the practical advantages of good form and good durable wearing quality; but, in view of actual existing conditions in the majority of engineering drawing offices, a design which is of simple and easy application on the drawing board has undeniable advantages.

The formulæ for the radii of curvature from the short shoulder arcs out to the point of the face and down to the root of the flank arc (see Appendix V.):—

Bottom Flank Radius

$$r/\mathrm{P} = 1\cdot103 + \frac{4800 - 335\cdot4\mathrm{N} + 10\mathrm{N}^2}{\mathrm{N}(\mathrm{N}-12)(\mathrm{N}-8)};$$

Normal distance of the Centre of the Bottom Flank arc from the tangent drawn to the pitch-circle at the shoulder of the tooth

$$d/\mathrm{P} = 0\cdot195 + \frac{1800 - 87\cdot5\,\mathrm{N} + 0\cdot6\,\mathrm{N}^2}{(\mathrm{N}-12)(\mathrm{N}-3)(\mathrm{N}+10)};$$

Top Face Radius

$$\mathrm{R}/\mathrm{P} = 1\cdot103 - \frac{8\,\mathrm{N} - 57}{(\mathrm{N}-8)(\mathrm{N}+23)};$$

Normal distance of the Centre of the Top Face arc from the above-mentioned tangent

$$D/P = 0 \cdot 195 + \frac{18 \cdot 7\,N - 228 + 0 \cdot 135\,N^2}{N(N+12)(N-11)}.$$

Although these are approximations with rounded-off figures for the constants, the minuteness of the errors of approximation are well illustrated by Table I. in Appendix V., which gives the true values of the chord from which the radius is calculated, its approximate value by approximation formula, and the error. This error is alternately + and − ; for the flank it is about +2 per cent. for 12 teeth, passes through zero at 14 teeth, and reaches an opposite maximum of $+1\frac{1}{2}$ per cent. at about 20 teeth, and then decreases to $+\frac{1}{2}$ per cent. for numbers of teeth somewhat over 100; for the face it is $-\frac{1}{4}$ per cent. for 12 teeth, passes through zero between 13 and 14 teeth, and reaches an opposite maximum of $+\frac{2}{3}$ per cent. at about 20 teeth, and then decreases to $+\frac{1}{8}$ per cent. for large numbers of teeth. The error of 2 per cent. at 12 teeth for the flank is of no importance whatever, because the flank is here straight, the radius is infinity, and any error in the arithmetical computation of infinity is of no effect, as the straight edge instead of the compasses are here used; besides, as has already been mentioned, it is not recommended to use pinions of less than 14, or at least 13 teeth.

Very long radii of curvature, when not infinite, i.e. for straight outline, are not only inconvenient in drawing and in gear-cutting, but it is also evident that a flat, or nearly flat flank cannot give a very close fit with a well-rounded face. This seems, at first sight, to offer an opening for criticism of the system as explained so far. But the flank radius, which is infinitely long for the 12-teeth pinion, decreases at an enormously rapid rate with rise of the number of teeth above 12; so that it is only $1 \cdot 55 \times$ pitch for 15 teeth, and, as noted above, is reduced to $0 \cdot 93\,P$ for 20 teeth. The suggested criticism is, therefore, of no practical effect.

All designs of wheel-teeth that have ever been proposed have resulted practically in certain circular arcs being substituted for the true theoretical curve of the tooth outline, and it seems probable that this will always remain so, except with one special form of rational gear-cutting machine. The author commenced this investigation by recognising this as a fundamental and unalterable fact of wheel manufacture; that is, he accepted as first principle that the outline is to be a series of circular arcs, two for face and two for flank being considered probably necessary and sufficient. From this starting-point he set out to discover which circular arcs gave the best results as

to (1) frictional efficiency; (2) abrasive wear, closeness of fit between face and flank being important for both these good qualities; and (3) uniformity of velocity-ratio, with proper gearing between any two of a complete set without special designs for this particular pair.

The avoidance of sharp shoulder curvature and advisability of close fit and little obliquity of thrust, lead to the adoption of the straight flank for the pinion of 12-teeth, this number being considered just below the smallest that can be recommended. The straight flank of this pinion defines the shape of the contact-path, and this again the correct theoretical radius of curvature for the tooth-outline for contact at each point of this path. These theoretically correct relations are as follows:—

$$\tau = 0 \cdot 153\,\mathrm{P}\left(\frac{\sin i}{0 \cdot 08} - 1\right) \quad \text{Polar equation to Path of Contact.}$$

$$\mathrm{R} = 0 \cdot 153\,\mathrm{P}\left(\frac{2n + 12}{\mathrm{N} + 12} \cdot \frac{\sin i}{0 \cdot 08} - 1\right)$$

$$r = 0 \cdot 153\,\mathrm{P}\left(\frac{2n - 12}{n - 12} \cdot \frac{\sin i}{0 \cdot 08} - 1\right)$$

where τ = radius vector from pitch-point to point of contact;
i = angle between τ and tangent at pitch-point;
R = radius for face of tooth for contact-point at τ, i;
r = „ flank „ „ „

N and n mean for any one wheel the same thing, namely, the number of its teeth; but as in the investigation of gearing it is always necessary to consider the flank of the tooth of one wheel in contact with the face of another wheel, n is used for the number of teeth of the wheel whose flank is in contact, and N for the teeth of the wheel whose face is in contact.

The various stages of the mathematical investigation by which this design has been evolved are given in the Appendices to the paper.

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APPENDIX I.

Rolling and Sliding.

The motion of one tooth upon another is a compound of pure rolling and sliding. The sliding component is of special importance as regards frictional waste of power and abrasive wear of the surfaces. The analysis of the motion into these two components is, therefore, of fundamental significance.

The laws of pure rolling are well known. The geometrical laws of the translational sliding of one curved surface over another are not, so far as the author knows, to be found in any of the well-known mechanical text-books. For the sake of comparison the two sets of laws are here stated together.

FIG. 1.—ROLLING: RELATIVE ANGLE OF ROTATION γ.

Convex on Convex: radii R *and* r.

(1) a = Rolled arc-length equal on each surface $= \gamma \dfrac{r\mathrm{R}}{r+\mathrm{R}}$

(2) a = angle on R-surface of motion of contact-point $= \gamma \dfrac{r}{r+\mathrm{R}}$

(3) β = „ r- „ „ „ „ $= \gamma \dfrac{\mathrm{R}}{r+\mathrm{R}}$

(4) $\gamma = a + \beta$

Convex R *on Concave* r.

(5) $a = \gamma \dfrac{r\mathrm{R}}{r-\mathrm{R}}$ (7) $\beta = \gamma \dfrac{\mathrm{R}}{r-\mathrm{R}}$

(6) $a = \gamma \dfrac{r}{r-\mathrm{R}}$ (8) $\gamma = a - \beta$

FIG. 2.—SLIDING: RELATIVE TANGENTIAL LINEAR TRANSLATION s.

Convex R *on Convex* r.

(9) a = motion of contact-point along R-periphery $= s \dfrac{\mathrm{R}}{r+\mathrm{R}}$

(10) b = „ „ „ „ r- „ $= s \dfrac{r}{r+\mathrm{R}}$

(11) $\alpha = \beta = \dfrac{s}{r + R}$

Convex R *on Concave* r.

(12) $a = s \dfrac{R}{r - R}$ in direction opposite to motion of r past R

(13) $b = s \dfrac{r}{r - R}$ „ same as „ R „ r

(14) $\alpha = \beta = \dfrac{s}{r - R}$

(15) Sliding distance determining frictional work for both convex on convex and convex on concave $= s$.

Fig. 3 helps to make the last case more easily understood. In it the small semicircle is supposed held fixed, while the larger semicircle slides on it parallel to itself, so that the touching point describes a complete semicircle on both fixed and moving arcs.

APPENDIX II.

General Equations of Uniform Velocity-Ratio.

Fig. 4 gives a demonstration of spur-teeth motion which is simple, and brings out the good or bad qualities of tooth-form. The line A B represents the fixed base or frame in which the two outside-geared wheels turn about the centres A and B. T t' represents a short arc-element of the two teeth where they touch. As the wheel A turns left-handedly through the very small angle α, the element T t' of its tooth-surface moves into the position $a' b'$, while at the same time the T t' element of the B tooth moves into the position $b' b''$, and the B wheel moves right-handedly round the centre B through the small angle β. Thus T $a' a$ is perpendicular to T A, and T $b' b$ to T B.

The motions being minute, $a' b'$ and $b' b''$ are both proximately parallel to T t', and therefore lie almost in the same straight line. N T n is drawn perpendicular to A B, and p T P to T t'. Simply for the sake of clearness, the small figure t' T $a' b' p'$ is enlarged to t T $a b p$, and n and p are the intersections on $a b$ of N T and P T. B B$''$ is drawn parallel to T t', and therefore perpendicular to P T, and B$''$ B$'$ perpendicular to T B.

The motion of the two wheels round the fixed base A B is looked on as the resultant of two successive motions. In the

first, the base along with the two wheels are rotated all together round centre A left-handedly through the angle a, there being no relative motion among the three bodies. In the second, while wheel A is held fast in its new position, the base is pulled back to its original position by a right-handed rotation a round centre A, dragging with it wheel B, so that this wheel's centre is brought back to its original position. In the first motion the touching element $\mathrm{T}\,t'$ of the B tooth is carried to $a'\,b'$. In the second motion it slides along $a'\,b'$ of the A tooth to the position $b'\,b''$, and at the same time the wheel B rotates round wheel A right-handedly through the angle $\gamma = a + \beta$. At the B end of the radius T B, the translatory or sliding motion is $\mathrm{B\,B''}$, and rotation γ moves the B wheel-centre through a path measured in length and direction by $\mathrm{B'\,B''}$. The sliding of tooth upon tooth is $a'\,b'$, or, on the enlarged scale, $a\,b$, while the rolling of tooth upon tooth is $\gamma = a + \beta$.

The point originally at P on wheel A has risen by the rotation a a height which we call p.

The triangle $\mathrm{T}\,a\,n$ is similar to A T P; triangles $\mathrm{T}\,b\,n$ and $\mathrm{B'\,B''\,B}$ are similar to B T P, and $\mathrm{T}\,p\,n$ to T N P. The angle P T N is called i, and the distance T P is called τ. In the second motion the rotation of wheel B takes place about its touching point T on wheel A, and therefore angle $\gamma = \mathrm{B''\,B'/T\,B}$.

Therefore,

$$\text{angle } a = \frac{\mathrm{T}\,a'}{\mathrm{TA}} = \frac{\mathrm{P}}{\mathrm{P\,A}} = \frac{\mathrm{B\,B'}}{\mathrm{B\,A}} = \frac{\mathrm{B'\,B''}}{\mathrm{TB}} \cdot \frac{\mathrm{P\,B}}{\mathrm{B\,A}} = \gamma \cdot \frac{\mathrm{P\,B}}{\mathrm{B\,A}}.$$

Therefore

$$\text{Angle } \beta = \gamma - a = \gamma\left(1 - \frac{\mathrm{P\,B}}{\mathrm{B\,A}}\right) = \gamma\,\frac{\mathrm{P\,A}}{\mathrm{B\,A}}.$$

Therefore $\qquad a/\beta = \mathrm{P\,B/P\,A}.$

Or, P divides the centre-line A B in the inverse ratios of the instantaneous angular velocities. Otherwise stated, P is the instantaneous "pitch-point," and the motion p of both wheels at P is the same. Also motion at pitch-point $= p = a \cdot \mathrm{A\,P}$ $= \beta \cdot \mathrm{B\,P} = \gamma\,\dfrac{\mathrm{A} \times \mathrm{B}}{\mathrm{A} + \mathrm{B}}$, where the pitch-radii of the two wheels are called A and B respectively.

The touching point T is shifted along the line P T outwards the distance $\mathrm{T}\,p'$, and at same time may be shifted right or left hand transversely to P T on the line $a'\,b'$. Therefore the increase of τ is

$$(1) \qquad \delta\,\tau = p\,\frac{\mathrm{A\,T}}{\mathrm{A\,P}} \cdot \frac{\mathrm{T}\,p}{\mathrm{T}\,a} = p\,\frac{\mathrm{T}\,a}{\mathrm{T}\,n} \cdot \frac{\mathrm{T}\,p}{\mathrm{T}\,a} = p \cos i.$$

Thus, $\mathrm{T}\,n$ and $\mathrm{T}\,p$ measure p and $\delta\,\tau$, while $\mathrm{T}\,a$ and $\mathrm{T}\,b$ measure the linear motions of the touching parts of the A and B teeth.

Also, since $a\,n/\mathrm{T}\,n = \tau/\mathrm{A}$, and $b\,n/\mathrm{T}\,n = \tau/\mathrm{B}$, therefore, $a\,n/b\,n = \mathrm{B}/\mathrm{A} = a/\beta$; and

$$(2) \qquad a\,b/\tau = \frac{\gamma}{\beta}\cdot\frac{b\,n}{\tau} = \gamma\frac{b\,n}{\tau}\cdot\frac{\mathrm{B}}{p} = \gamma.$$

Thus $a\,n/\tau$, $b\,n/\tau$, and $a\,b/\tau$ measure the angles a, β, and γ, and the

$$(3) \text{ Sliding of tooth on tooth} = s = a\,b = \tau\gamma = p\frac{\tau\,(\mathrm{A}+\mathrm{B})}{\mathrm{A}\times\mathrm{B}}.$$

In the first of the supposed component motions, that is, before there was any sliding or rolling of tooth on tooth, the contact-point was carried from T to a, the distance $a\,n = \tau\,a = p\frac{\tau}{\mathrm{A}}$ to the left of the vertical N T normally to P T. The radius of curvature of the convex face of the A tooth being called R, and the radius of curvature of the concave flank of the B tooth being called r, the right-handed sliding $\tau\,\gamma$ carries the touching point still further along $b\,a$ to the left, the distance $\tau\gamma\frac{\mathrm{R}}{r-\mathrm{R}}$. But the rolling γ carries it back towards the right by the distance $\gamma\frac{r\,\mathrm{R}}{r-\mathrm{R}}$, where $\gamma = a + \beta$. Adding these three parts together we find the distance of the new touching point from n to the right, measured perpendicularly to P T, or along the line $a\,b$, to be

$$\gamma\,\mathrm{R}\frac{r-\tau}{r-\mathrm{R}} - p\frac{\tau}{\mathrm{A}} = p\frac{(\mathrm{A}+\mathrm{B})\,\mathrm{R}\,r - (\mathrm{A}\,\mathrm{R}+\mathrm{B}\,r)\tau}{\mathrm{A}\times\mathrm{B}\,(r-\mathrm{R})}.$$

The last form of this expression is symmetrical with regard to the A and B wheels.

The distance $n\,p$ of point n from line P T is $p \sin i$; and, if the above were equal to $p \sin i$, the contact-point T would simply move outwards $\delta\,\tau = p \cos i$ along the line P T.

By subtracting $p \sin i$ and dividing the whole by $p \cos i$, we have the tangent of the right-hand outward inclination of the path of contact from P T. In Fig. 5 this angle is shown and called θ, a short element of the contact-path at T being called T T′. Thus $(i + \theta)$ is the angle of inclination of this part of the contact-path from the vertical P P″ or N T; and

$$\tan i + \tan\theta = \frac{(\mathrm{A}+\mathrm{B})\,\mathrm{R}\,r - (\mathrm{A}\,\mathrm{R}+\mathrm{B}\,r)\,\tau}{\mathrm{A}\times\mathrm{B}\,(r-\mathrm{R})\cos i}.$$

At the pitch-point $\tau = 0$ and $\theta = 0$.

If i_0 be the angle at which the curve crosses at P, the above gives

$$\frac{R_0 \, r_0}{r_0 - R_0} = \frac{N \, n}{N + n} \sin i_0 \, . \, \frac{p}{2\pi}$$

where the suffixes o to R_0 and r_0 indicate that these are the radii of face and flank curvature at the pitch-line, and where N is the number of teeth of the wheel whose tooth-face touches the tooth-flank of a wheel of n teeth.

APPENDIX III.

Equations with Straight Flank in 12-*teeth Pinion.*

If N_1 be number of teeth in the smallest pinion, the angular pitch of this pinion is $\frac{360°}{N_1}$; its pitch-circle circumference is $N_1 P$; and its diameter $\frac{N_1 P}{\pi}$, where P is the peripheral pitch. If the thickness of its tooth were made parallel below the pitch-circle, the flanks on each side of a gap between two teeth would converge at an angle $\frac{360°}{N_1}$, and if the flanks were made radial they would converge at an angle a little greater than $\frac{180°}{N_1}$. In the latter case the path of contact would cross the line of centres at 90°, and the flank gap of the rack would be parallel; also the radius of curvature of the teeth of all sizes of wheels at the pitch-circle would be zero. In the former case the contact path would pass the pitch-point at the angle $\frac{90°}{N_1}$, or $\frac{\pi}{2\,N_1}$ in circular measure.

As a compromise between sharpness of curvature at the shoulder and excessive obliquity of driving thrust, the contact path is made to pass the pitch-point at an angle $i_0 = \eta \, \frac{\pi}{2\,N_1}$ $= 0 \cdot 612 \, \frac{\pi}{2\,N_1}$ to the tangent. With $N_1 = 12$, $\sin i_0 = 0 \cdot 08$

exactly, and $\frac{P}{2\pi}\sin i_o = \frac{\eta P}{4 N_1} = 0\cdot 153 \frac{P}{N_1}$. This makes the final equation (5) of Appendix II.

$$(1)\quad \frac{R_0 r_0}{r_0 - R_0} = \frac{\eta P}{4} \cdot \frac{N n}{N_1 (N + n)} = 0\cdot 01275\, P \frac{N n}{N + n}.$$

This applied to the case of the gearing of two racks, or two equal very large wheels, gives $R_\infty = r_\infty$. Other considerations also show that the face of the rack-tooth is throughout the exact reverse of its flank.

Applied to the gearing of two pinions each of N_1 teeth, it yields $R_1 = \eta P \frac{r_1}{\eta P + 8 r_1}$, and if the flank of the N_1 pinion be made straight, or $r_1 = \infty$, this means

$$(2)\qquad R_1 = \frac{\eta}{8} P = 0\cdot 0765\ P.$$

Apply it next to the case of $N = N_1$ and $n = \infty$, or pinion of N_1 teeth gearing with a rack. The result is

$$R_1 r = (r_\infty - R_1) \frac{\eta P}{4}.$$

$$(3)\qquad \therefore\quad r_\infty = R_\infty = \frac{\eta}{4} P = 0\cdot 153\ P.$$

Apply it to the case of the face of a wheel of any number of teeth, N gearing with the flank of the rack-teeth :—

Face shoulder for N teeth

$$(4)\qquad R_0 = \frac{\eta P}{4} \cdot \frac{N}{N + N_1} = 0\cdot 153\, P \frac{N}{N + 12}.$$

And again, to the flank of same wheel-tooth gearing with the rack-tooth face :—

Flank shoulder for n teeth

$$(5)\qquad r_0 = \frac{\eta P}{4} \cdot \frac{n}{n - N_1} = 0.153\, P \frac{n}{n - 12}.$$

From this formula would also follow the measure of the "closeness of fit" for contact close to the shoulders of wheels of N and n teeth :

$$(6)\quad \frac{1}{2}\left(\frac{1}{R_0} - \frac{1}{r_0}\right) = \frac{2}{\eta P} \cdot \frac{N_1 (N + n)}{N n} = \frac{39\cdot 2}{P} \cdot \frac{N + n}{N n}.$$

The shoulder radii actually used, however, are greater than the above, and the fit is thus much closer.

Except for the choice of $\sin i_o = 0 \cdot 08$; $N_1 = 12$; and $r_1 = \infty$, all in this Appendix is independent of the special shape chosen for the contact-path.

The difficulty in choosing the base elements is to combine avoidance of convexity in the flank with avoidance of sharp curvature in the face; because the above fundamental shoulder equation also gives

$$r_1 = \frac{\eta P R_\infty}{\eta P - 4 R_\infty},$$

$$r_\infty = \frac{\eta P R_1}{\eta P - 4 R_1},$$

which show that if either R_∞ or R_1 be made greater than $\frac{\eta}{4} P$, then the flank radius at shoulder becomes negative, which means convexity. This is a basic difficulty in all "universally interchangeable" designs whatever contact-path be chosen. Fig. 6 shows the variation of r_1 and $R_\infty = r_\infty$ through the total possible range of choice of R_1. The diagram shows that no choice of R_1 much different from $\frac{1}{8}$ P is theoretically correct for exact uniformity of velocity ratio.

APPENDIX IV.

Contact Path. Radii for Outline. Closeness of Fit.

In Fig. 10, which shows the outline of the 12-teeth pinion, there is also inscribed the contact-path. It is determined by the flatness of the flank of this 12-teeth pinion. If i_o were made zero, it would be the arc of a circle of diameter equal to B_1, the radius of this pinion. But, as seen in Fig. 10, the straight flank, instead of running to the centre of the pinion, lies tangent to a small circle round this centre of radius $B_1 \sin i_o$. If through the centre be drawn a line parallel to the flank, and remembering that $B_1 = \frac{\eta}{4 \sin i_o}$ P, it is easily recognised that the polar

equation to the contact path is

$$(1)\qquad \tau = B_1(\sin i - \sin i_0) = \frac{\eta P}{4}\left(\frac{\sin i}{\sin i_0} - 1\right),$$

from which is at once deducible

$$(2)\qquad \tan\theta = \tau\frac{di}{d\tau} = \frac{\sin i - \sin i_0}{\cos i}.$$

Inserting this value in the general equation (4) Appendix II., the following relation is found between R for the face with N teeth gearing with r of the flank with n teeth, at each point of the contact-path defined by sin i.

$$(3)\qquad \frac{4 N_1 (N+n)}{\eta P . N n} = \frac{N + N_1}{R N}\left\{\frac{2N + N_1}{N + N_1}\cdot\frac{\sin i}{\sin i_0} - 1\right\}$$
$$- \frac{n - N_1}{r n}\left\{\frac{2n - N_1}{n - N_1}\cdot\frac{\sin i}{\sin i} - 1\right\}.$$

Apply this successively to the cases,

(1) N any number, $n = N_1$ with $r_1 = \infty$,
(2) $N = \infty$ and $n = N_1$;
(3) $N = \infty$ and n any number;
(4) $N = N_1$.

There result the following set of formulas, which give a complete theoretical account of this system of interchangeable toothed-wheels:—

$$N_1 = 12 : \eta = 0\cdot612 : \frac{\eta}{4} = 0\cdot153 : \frac{\sin i}{\sin i_0} = i'.$$

(4)	$\tau = 0\cdot153\,P(i' - 1)$	contact-path.
(5)	$R = 0\cdot153\,P\left(\frac{2N + 12}{N + 12}\cdot i' - 1\right)$	face-radius for N teeth.
(6)	$r = 0\cdot153\,P\ \frac{2n - 12}{n - 12}\cdot i' - 1$	flank-radius for n teeth.
(7)	$r_1 = \infty$	straight flank for 12 teeth.
(8)	$R_1 = 0\cdot153\,P\left(\frac{3}{2}i' - 1\right)$	face-radius for 12 teeth.
(9)	$R_\infty = r_\infty = 0\cdot153\,P(2i' - 1)$	face and flank radius for rack.

(10) $$R_0 = 0\cdot153\,P\,\frac{N}{N+12}$$ shoulder face-radius for N teeth, $i' = 1$.

(11) $$r_0 = 0\cdot153\,P\,\frac{n}{n-12}$$ shoulder flank radius for n teeth, $i' = 1$.

(12) $$R_5 = 0\cdot153\,P\,\frac{9\,N+48}{N+12}$$ face radius at end of contact for N teeth, $i' = 5$

(13) $$r_5 = 0\cdot153\,P\,\frac{9\,n-48}{n-12}$$ flank radius at end of contact for n teeth, $i' = 5$

Closeness of fit between face of N teeth and flank of n teeth:

(14) $$\tfrac{1}{2}\left(\frac{1}{R}-\frac{1}{r}\right)=\frac{1}{0\cdot306\,P}\left\{\frac{N+12}{(2\,i'-1)\,N+(i'-1)\,12}-\frac{n-12}{(2\,i'-1)\,n-(i'-1)\,12}\right\}$$

Ditto, for shoulder to shoulder contact, $i' = 1$;

(15) $$\tfrac{1}{2}\left(\frac{1}{R_0}-\frac{1}{r_0}\right)=\frac{39\cdot2}{P}\cdot\frac{N+n}{N\,n}$$

Ditto, for point to root contact, $i' = 5$;

$$\tfrac{1}{2}\left(\frac{1}{R_5}-\frac{1}{r_5}\right)=\frac{2\cdot42}{P}\cdot\frac{N+n}{(N+5\frac{1}{3})\,(n-5\frac{1}{3})}.$$

APPENDIX V.

Circular Arc Approximations to true Outline.

The values found in Appendix IV. for τ, R and r all increase uniformly with i' or $\sin i/\sin i_0$, which ranges from 1 at the pitch-point to $\cdot5$ at the ends of the contact-path, while $\sin i$ varies nearly in proportion to i, because i does not range beyond $23\frac{1}{2}°$. Again $(i - i_0)$ is strictly proportional to the peripheral arc rolled through from pitch-point on the pitch-circle of any wheel of any number of teeth. In fact $(i - i_0) = \frac{\pi}{6\,P}\cdot p$ in

circular measure. This proximate uniformity of increase with the arc-length on the pitch-circle justifies methods of approximation to the exact tooth-outline by circular arcs. In selecting such methods, however, it must be remembered that the arc-lengths along the outline of the tooth do *not* vary at all in the same uniform manner. Therefore no arithmetical mean of the radii taken with respect to p, or $(i - i_0)$, or sin i, give satisfactory approximations.

The approximations adopted are two very short shoulder arcs on face and flank sides of pitch circle, and two much longer completing arcs running tangentially into the shoulder arcs, and giving both true position and true direction at the point of the face and at the root of the flank.

The short shoulder arcs range from $i' = 1$ to $i' = 2$, giving true position and true direction at each end of this range. An exact trigonometrical calculation shows that the correct radii are the arithmetical means of the theoretical radii at pitch point and at $i' = 2$. Equations (5) and (6) Appendix IV. give these:—

(1) $$R_0 = 0 \cdot 306 \text{ P} \frac{N + 3}{N + 12}.$$

(2) $$r_0 = 0 \cdot 306 \text{ P} \frac{n - 3}{n - 12}.$$

The distances of the centres of these arcs from the tangent at pitch-point are $0 \cdot 08$ of the radii, since they lie on the line at i_0 to this tangent, or

(3) $$D_0 = 0 \cdot 0245 \text{ P} \frac{N + 3}{N + 12}.$$

(4) $$d = 0 \cdot 0245 \text{ P} \frac{n - 3}{n - 12}.$$

The completing arcs run from $i' = 2$ to $i' = 5$. At $i' = 5$, equation (4) of Appendix IV. gives

$$\tau = \frac{\eta}{4} \text{P} \;.\; 4 = \eta \;.\; \text{P} = 0 \cdot 612 \text{ P}.$$

The distances of points within the tooth-outline by R and r_0 from the pitch-point are, therefore, now

(5) $$\tau - R_0 = 0 \cdot 306 \text{ P} \frac{N + 21}{N + 12}.$$

(6) $$\tau - r_0 = 0 \cdot 306 \text{ P} \frac{n - 21}{n - 12}.$$

When $n > 21$, the point so found for the flank is outside the pitch-circle.

Fig. 8 shows the construction for finding the radii and centres of the completing arcs, the centre of a shoulder arc being marked C, and that of the corresponding completing arc, K. T is the touching point when $i' = 5$, and P the pitch-point. The angles used in the construction are explained in Fig. 7. β is the angle through which the wheel has revolved up to $i' = 5$ from the passage of the shoulder through P. Fig. 7 shows that T P or τ makes with the tangent at the shoulder the angle $(i + \beta)$ for the face and $(i - \beta)$ for the flank; while with the tangent to the contact-path at P (which makes with the tangent to the pitch-circle the angle i_0) τ makes the angles $(i - i_0 + \beta)$ for face and $(i - i_0 - \beta)$ for flank. With the chord from shoulder to P, the line τ makes the angles $\left(i + \frac{\beta}{2}\right)$ for face and $\left(i - \frac{\beta}{2}\right)$ for flank. From T, Fig. 8, is plotted along τ towards P the radius R_0 for face, and r_0 for flank, and this point is marked c. The centre K must be equidistant from C and c, and must lie on the prolongation of τ. Therefore the angle c C K is made equal to the angle C c K, and the length of chord C c is calculated. The calculations of angle C c K and of chord C c are both very complicated and tedious, and need here be indicated only in outline. The length of chord S P is easily found, and those of P c = $(\tau - R_0)$ for face and $(\tau - r_0)$ for flank are given above. The vector resultant of these is C c, and the angles they make with S C are known. From their projections parallel and perpendicular to S C, the length C c is found, and at the same time the ratio of these components of C c gives the tangent of the inclination of C c to S C. From this last angle that between C c and τ is calculated.

Thus calculated, the chord C c has the length

$$(7) \quad C c = P\left(0{\cdot}15337 \mp \frac{0{\cdot}944}{n} - \frac{0{\cdot}1754}{n^2} \mp \frac{7{\cdot}32}{n^3} - \frac{11{\cdot}01}{n^4}\right)$$

where the upper $-$ sign is for flank, and the lower $+$ sign for the face. An approximation to this is

$$(8) \qquad C c = P\left(0{\cdot}153 \mp \frac{1{\cdot}12}{n} + \frac{2{\cdot}4}{n^2}\right)$$

The error in this approximation is illustrated in the subjoined table :—

Number of Teeth		Approximate 3-term formula	Error	True Value
12	Flank	0·0764	+0·0016	0·0748
	Face	0·2630	−0·0006	0·2636
15	Flank	0·0890	−0·0005	0·0895
	Face	0·2384	+0·0007	0·2377
20	Flank	0·1030	−0·0015	0·1045
	Face	0·2150	+0·0013	0·2137
50	Flank	0·1315	−0·0013	0·1328
	Face	0·1763	+0·0007	0·1756
100	Flank	0·1420	−0·0008	0·1428
	Face	0·1644	+0·0002	0·1642

A similarly close approximation to the circular measure of the angle $\mathrm{C}\,c\,\mathrm{K}$ is

$$\mathrm{C}\,c\,\mathrm{K} = \frac{\pi}{2} - \left(0\cdot1236 \mp \frac{1\cdot135}{n} - \frac{4\cdot18}{n^2}\right)$$

the coefficients being adjusted so as to give the exact true values for both face and flank at the extreme limits of the rack and the 12-teeth pinion, and the errors for intermediate members of teeth being negligibly minute.

The distance $\mathrm{K\,C} = \mathrm{K}\,c = \frac{1}{2}\,\mathrm{C}\,c \div \sin\frac{\mathrm{C\,K}\,c}{2}$; and this angle $\frac{\mathrm{C\,K}\,c}{2}$ is only 7° 5′ for face and flank of the rack; is 0 for flank, and 10° 51′ for the face of the 12-teeth pinion; so that approximation in making this division is quite allowable. To $\mathrm{K}\,c$ has to be added R_0 or r_0 as already found in order to obtain $\mathrm{K\,T} = \mathrm{R}$ or r for the completing arc.

After adjusting the factors so as to obtain almost exactly true values throughout the whole range from the 12-teeth pinion

to the straight rack, the resulting formulæ for the radii of the arc for flank and face are as follows:—

(10) Top face radius $R = P\left\{1 \cdot 103 - \frac{8\,N - 57}{(N-8)(N+23)}\right\}$;

(11) Bottom flank radius $r = P\left\{1 \cdot 103 + \frac{4800 - 335 \cdot 4n + 10n^2}{n\,(n-12)\,(n-8)}\right\}$.

To find the exact positions of the centres K of these arcs, the method of finding the angles between the various lines in the figure has already been explained.

The angle which the line C K joining the centres of the shoulder and the completing arcs makes with the tangent to the pitch-circle at the shoulder S is

(12) for face arc $0 \cdot 1641 + \frac{1 \cdot 7}{N} + \frac{9 \cdot 36}{n^2}$

(13) and for flank arc $0 \cdot 1641 - \frac{1 \cdot 7}{n} + \frac{9 \cdot 36}{n^2}$

The size of this angle multiplied by $(R - R_0)$ for face, and by $(r - r_0)$ for flank and this product added to the normal distance of C from the tangent at S (which last has already been given in equations (3) and (4) of this Appendix), is the normal distance of K from the same tangent. This distance, called D for face and d for flank, is given in Fig. 9 by two curves for all sizes of wheel.

Two formulæ of the same general character as those for the radii have been worked out for these distances D and d, the factors being adjusted so as to give accurate values throughout the whole range. These are

(14) $D = 0 \cdot 195 + \frac{18 \cdot 7\,N - 228 + 0 \cdot 135\,N^2}{N\,(N+12)\,(N-11)}$

and

(15) $d = 0 \cdot 195 + \frac{1800 - 87 \cdot 5\,n + 0 \cdot 6\,n^2}{(n-12)\,(n-3)\,(n+10)}$.

This normal distance of the centre from the tangent to the pitch circle drawn at the shoulder to the tooth is the most accurate and the easiest draughting method of locating these centres.

The following table gives examples of the radii and centre distances calculated as above:—

RADII AND CENTRE-DISTANCES, FOR PERIPHERAL PITCH = 1.

Number of Teeth	12	20	30	100	Rack
R	·191	·219	·240	·281	·305
D_0	·0153	·0176	·0192	·0225	·0244
r_0	∞	·649	·458	·337	·306
d_0	∞	·0519	·0366	·0269	·0244
R	·825	·903	·946	1·036	1·103
D	·250	·230	·214	·198	·195
r	∞	2·19	1·42	1·191	1·103
d	∞	·266	·180	·194	·195

APPENDIX VI.

Frictional Efficiency.

The frictional waste of power is calculated in the following manner:—

The linear velocity of sliding of tooth over tooth has been shown to be $\tau\gamma' = \tau(a' + \beta')$, where τ' and β' are the angular velocities of the two wheels respectively to the frame in which they are centred, both being reckoned +, although they are of opposite rotative signs. γ' is unaffected by the shapes of the teeth. τ ranges from 0 as the touching-point passes pitch-point up to its value, when $i' = 5$ at end of contact. Both the method in which it varies throughout this range, and its extreme value depend upon the shape of contact-path chosen and the length of contact. The smaller i_0 is made, the more rapid is the increase of τ from zero, and the greater is its average value in proportion to its extreme limiting length.

The obliquity of action, and the ratio of the friction generating thrust to the useful driving component of this thrust, also increase with i_0 and with the general inclination of the contact-path to the tangent at pitch-point. Thus what one gains in less obliquity with a small i_0, is largely lost in larger average value of τ, and the correct choice of i_0 is a problem of the minimisation of the product of two evil influences.

With a useful driving thrust F parallel to tangent at pitch-point, the actual thrust between teeth is F/cos i when the touching-point T has the polar co-ordinates i and τ; and since $\tau = \frac{\eta P}{4}\left(\frac{\sin i}{\sin i_0} - 1\right)$, therefore, with friction co-efficient f, the rate of doing work on friction is

$$f F \frac{\eta P}{4} \left(\frac{\sin i - \sin i_0}{\sin i_0 \cos i}\right)(\alpha' + \beta').$$

Here α' is the differential of $\frac{12}{N}(i - i_0)$ and β' of $\frac{12}{n}(i - i_0)$ so that $(\alpha' + \beta')$ is the differential of $12 \frac{N+n}{Nn}(i - i_0)$ or $12 \frac{N+n}{Nn} \delta i$. Since $\frac{\eta}{4} = 0 \cdot 153$ and $12 \times 0 \cdot 153 = 1 \cdot 836$, the frictional work done while i increases from i_0 to any value i is the integral between these limits of

$$(1) \qquad 1 \cdot 836 f F P \frac{N+n}{Nn} \cdot \frac{\sin i - \sin i_0}{\sin i_0 \cos i} \delta i.$$

This integral is

$$(2) \qquad 1 \cdot 836 f F P \frac{N+n}{Nn} \left\{ \frac{1}{\sin i_0} \log_e \frac{\cos i_0}{\cos i} - \log_e \left(\frac{1 + \sin i}{1 - \sin i} \cdot \frac{1 - \sin i_0}{1 + \sin i_0}\right) \right\}$$

In some cases the contact lasts $\frac{2}{3}$ P or somewhat beyond sin i = 5 sin i_0, and in others considerably less than this. The variation is not large, and as the friction co-efficient f cannot be known very accurately, it is sufficient to calculate an average integral up to sin i = 5 sin i_0. Up to this limit from pitch-point, the integral frictional work is

$$(3) \qquad 1 \cdot 836 f \mathrm{F\,P} \frac{\mathrm{N}+n}{\mathrm{N}\,n} \left\{ \frac{0 \cdot 084}{0 \cdot 080} - 0 \cdot 687 \right\}$$

$$= 1 \cdot 836 \times 0 \cdot 363 f \mathrm{F\,P} \frac{\mathrm{N}+n}{\mathrm{N}\,n} = \frac{2}{3} f \mathrm{F\,P} \frac{\mathrm{N}+n}{\mathrm{N}\,n}.$$

The useful work done during the same time is $0 \cdot 63$ P F. Therefore the ratio of

$$(4) \qquad \frac{\text{frictional waste horse power}}{\text{useful effective horse power}} = 1 \cdot 06 f \frac{\mathrm{N}+n}{\mathrm{N}\,n}.$$

Therefore, as ordinarily reckoned, the

$$(5) \qquad \text{frictional efficiency} = \frac{1}{1 + 1 \cdot 06 f \frac{\mathrm{N}+n}{\mathrm{N}\,n}}.$$

DISCUSSION.

The Chairman moved a vote of thanks to the author for his paper, and, in doing so, said those present would agree with him that the author had demonstrated once more the value of ripe theoretical experience brought to bear on practical questions. They knew that at the present time there was great need for such a paper, in order that the subject might be brought up to date. He (the Chairman) could remember very well working in shops forty years ago where one of the chief features was the making of teeth, and he was quite sure that no such trouble as the author had indicated was ever taken in order to define so accurately and carefully the shape that they should follow. The question was one which lent itself to a special form of discussion, and he was glad that there were present at the meeting those who would be able to help in this matter. He was sure that they would all thank the author very heartily for his valuable paper.

The motion was carried with acclamation.

Mr. Archibald Sharp said he thought that Professor Smith was to be congratulated on having found something new to say about wheel teeth. He (the speaker) remembered that, when he wrote a paper some twelve years ago on wheel teeth, he was told by a well known authority on the subject that there was nothing new to be written or to be found out about wheel teeth. Their old friends, the cycloidal wheel teeth, had been out of fashion for some time, and the involute form of tooth had had all its own way, at least with machine-cut gear wheels, but the point of closeness of fit, he had no doubt, would be of great practical importance. He thought that it was a pity that the author in his paper had not given some notion of the relative values to be placed on closeness of fit and other good

qualities of teeth. Many gears which were every-day things in automobile work ran, if properly treated, for tens of thousands of miles without giving any indication of wear. After having spent a good deal of time on the mathematics of wheel teeth, he had come to the conclusion that the shape of the tooth did not matter much, provided that the tooth outlines were "fair," that the shape of each tooth in each wheel was exactly the same, and that the spacing was accurate. In his own paper he set out to design tooth outlines composed of a circular arc for each tooth, instead of the four circular arcs which Professor Smith suggested. He found that, if the radius of curvature and the position of the centre of curvature were chosen properly, taking a 13-tooth pinion gearing with a 52-tooth wheel, the percentage variation of speed-ratio was only 0·47 of 1 per cent. Further, 1 per cent. variation of speed, with a tooth of 1-inch pitch, corresponded to a deviation from the true mathematical outline of less than $\frac{1}{1000}$th part of an inch, and so, he thought, any errors of velocity ratio, due to design of circular wheel teeth, were far less than workshop errors due to unequal spacing of wheel teeth. He did not know what the limit of accuracy in spacing machine-cut wheel teeth was, but he had often wished that the National Physical Laboratory would make a report on the matter. He wished that they would take one of the best cut gear wheels made, and let engineers have a notion of the magnitude of the errors.

It seemed to him an injustice had been done to involute teeth. An involute tooth had a straight line path of contact. They were at perfect liberty to choose the angle of obliquity of the path of contact. In fact, in all systems of designing tooth wheels to satisfy the condition of uniform velocity ratio, there was one factor which could be chosen arbitrarily. If they took the path of contact almost coincident with the tangent, a very short tooth was obtained, and there would be a risk of disengagement from elastic yielding of shafts and supports. If a big angle were taken—fifteen degrees was the angle that had often figured in text books on the subject—a fairly robust form of tooth was obtained. If they set out with the condition that the two tooth outlines should be circular arcs, the complete path of contact with only one complete circular pin on each wheel was a lemniscate (a "figure of eight" curve), but the portion used in an actual wheel was the short part in the neighbourhood of the pitch point, and that practically was a straight line, and so he could not quite see why the properties of involute and circular wheel teeth should be so divergent.

The author had said, "The radius of curvature of the tooth outline close to pitch point must be zero for maintenance of uniform velocity ratio. This is true, whatever shape of contact path be adopted. It means a theoretical sharp knife-edge

shoulder to the tooth, and this is not only impossible of manufacture, but would also produce extravagantly rapid wear." He (the speaker) thought that that was perhaps too strongly stated. It was true that with cycloidal tooth outlines the radius of curvature at the pitch line was zero, but that zero curvature only extended zero distance along the face of the tooth.

With circular wheel teeth they could assume three points at which the actual velocity would be exactly equal to the theoretical velocity, so that, with the author's four arcs, he, possibly, was able to get that twelve times. With regard to Appendix I., he (the speaker) found, some years ago, the same difficulty to which the author had referred. He hunted in all the books which he had, and he found nothing upon the point which he wanted. He had discussed the equations of relative motion of two bodies in contact, on pages 34–38 of his book "Bicycles and Tricycles."

Mr. J. M. Strachan said the teeth of wheels were like those things which people looked at, and as to which they said, "It is a pretty thing; it is very pretty," but, with regard to which they forgot to go into that fine detail which gear wheels ought to possess. The author had brought forward a tooth, the shape of which resembles one which Mr. Humphris had been working on for the last eleven years. Mr. Humphris had found that, by making a circular tooth and making a round hole in a rack, he could make a tooth a great deal stronger than the ordinary machine-cut tooth, and that he could get an area of contact with a circular tooth which seemed almost an impossibility, but it was so. Then Mr. Humphris could run a 6-tooth pinion in a rack at a correct velocity ratio, and, with a tooth an inch wide, he could transmit about five times as much power through that thickness of wheel as he could with an ordinary machine-cut tooth on the Brown and Sharpe formula, which is quite an impossibility with ordinary cut gears with a smaller number of teeth than 12 in the pinion. The contour of the tooth was the same as the author's, but it had a different shape, the tooth being round, or of an oval section, the hole being completely circular, and the teeth being a different size. The tooth was 0·7 inch of the pitch, and the space was 0·3 inch of the pitch. It was a hole bored out in a piece of metal, and a round tooth was made to go into a hole. It could be put in to the accuracy of 0·001 of an inch, and it rolled and slid the same as any other tooth, but the slip was much less than in an ordinary tooth. The contour of the tooth was the same as the lecturer's, only the sizes of teeth being different in relation to one another. The angle of obliquity to prevent crowding of the bearings with a rack and a tooth came out at 6 degrees instead of 14½, which is the standard Brown and Sharpe tooth. The

angle of obliquity was only 6 degrees; and the number of effective contacts was less, owing to the teeth being larger. The Adams Manufacturing Company, of Bedford, made epicyclic gear boxes. They gave a great deal of trouble, because there were so many wheels in them, and the planetary wheels had to go at such a tremendous speed before the change that was wanted could take place. When those gear wheels were set going at a high speed, a great amount of loss arose through the "makes and breaks" that had to take place, and the displacement of the oil. The teeth had to force the oil out of the spaces, and, when this occurred at an enormous speed, the resistance was considerable. If anyone looked up Grant's book on gearing, he would see that, after the tooth made so many contacts, the efficiency began to go down. That was the cause of a lot of lost power in motor cars—the high speed the fine-cut gears had to go. The gears in the epicyclic boxes were cut very accurately. His firm cut them to 0·0015 of an inch on the corrected pitch line. On placing orders for quantities of gears, the manufacturers undertook that they would make them within 0·0015 of an inch, but they did not come anywhere near that, and the speaker's firm had to cut their own, using a Brown and Sharpe gauge to measure the corrected tooth. Mr. Humphris claimed that he got more power with his type of tooth, owing to the angle of pressure being less. When tooth wheels came together at a wide angle, a great deal of rubbing took place, until they came down to the pitch point. He had found that the cutters made by the Brown and Sharpe Company were very accurate. Measuring all the way round on the corrected pitch line, it would be found that there was not as much as 0·0005 of an inch error.

Mr. F. Humphris said that, with regard to ordinary tooth gears, he had had an experience ranging over twenty years in manufacturing from the smallest watch-wheel up to the largest possible wheel that had ever been made, and, as a result of his experience, he had found out that what the author had said that evening was absolutely true. He agreed with the author in every detail. There was not one single point or formula from which he dissented. The only thing he wished to say was, that the author had not gone as deeply into the subject as he would go a little later as the result of what he had discovered up to the present! The discovery that had been made by the author was a discovery which was priceless to the engineering world. The engineering world, up to the present time, had rather scoffed at the idea of changing the form of tooth from the existing involute form, or having epicycloidal or cycloidal addendum and dedendum teeth, and it used a tooth which should have been abandoned years ago. The reason why it should

have been abandoned was exactly what the author had said, namely, that the engaging area of that particular tooth must be, if it was theoretically correctly formed and perfect, only a line of contact. The resistance to abrasion of a line must be very small indeed, and, if it were a knife-edge engagement, and the teeth were made of diamonds, they could not remain the correct shape for a very great length of time. The action of the teeth of wheels, or the action of one tooth upon another, was an action of sliding contact the whole of the time, excepting at the moment when the tooth passed the pitch point; and, as a result of that sliding action, if the teeth are constructed in accordance with Professor Smith's design, the greater the area became, the more lasting and the more nearly perfect the tooth would be, and the better would it receive the lubricant and remain in shape. Mr. Archibald Sharp had spoken with reference to the ordinary form of tooth gear and the life of a particular wheel in a motor car. If the wheel had been made from the material engineers had fifteen to twenty years ago, that wheel would not have stood the load for three weeks; it would not stand a thousand miles running. The metallurgist had come to the aid of the engineer, who had been too lazy or indifferent to turn his mind to the important matter of wheel transmission, and he had brought out something which had made it possible for the engineer to run a motor vehicle at all. The ordinary motor omnibus was simply a box of mystery, and a snorting, grunting leviathan which was thrown together and manufactured by people many of whom should not claim to be engineers. If the author's form of teeth were fitted into one of the vehicles which he had mentioned, the result would be a more silent vehicle and a far more durable transmission. The truth of that statement would be evident to any engineer who had ever, in any way, studied the form of tooth gears, and to anyone who had gone further, and studied the sliding action of two surfaces one upon another.

As to the materials used at the present time in the ordinary motor cars which made it possible to run them—and he particularly emphasised the point—if Professors Willis and Unwin and others who had made it their business to design and work out the form of tooth wheels or teeth, or odontoids, had had those materials, it would have been possible to design teeth very much smaller than the existing teeth used in the motor vehicle of to-day. He would like to state how the material was really found. The facts were not generally known even to the engineering world. When motor vehicles were first made, the most difficult part of the whole design lay in the fact that they were too heavy, and the amateur condemned the motor vehicle on the ground of its weight, and also upon the ground of its

noise, and that it was always breaking. That was before the high grade steels were invented, and when ordinary material was used. It was then found necessary to put in very large and wide teeth. The agents dealing in motor vehicles were continually drumming it into the manufacturers that their material was no good. As a matter of fact, it was not so much the material as the design ; but the manufacturer at that time, still being ignorant, and thinking that it was his material, spoke to the people who supplied him with the steel, and they, in turn, endeavoured to find a material which should give better results. What did all this come to ? It simply brought us to this one point, that, if we had a tooth-wheel such as Mr. Sharp had referred to, which, in conjunction with other wheels in the gear-box, would run ten thousand miles, having the ordinary involute form of tooth, but being made of the highest grade nickel chrome steel—if that wheel, then, would run ten thousand miles, and show no signs of wear, that was no proof whatever that the gear was an efficient one, and it established no claim whatever that the ordinary gear was something of which engineers could be proud. To simply stick to an old form of tooth because it had been used by our grandfathers, and to maintain that that form of tooth was right, was simply ridiculous. It was with the greatest degree of pleasure that he had come to the meeting to listen to the author's very able paper. The paper clearly showed that, to those who would only open their ears and be prepared to receive the valuable matter which the author had given them, it was possible to improve the ordinary form of tooth. Many people were rather inclined to scoff at the idea that it was possible to improve the ordinary form of tooth-gear, and it required a person with more than ordinary courage to come and claim that he could improve the ordinary tooth-gear. Many were inclined to listen to a lecture, and say, " I think nothing of it " ; but, as a person who had had, possibly, more practical experience in tooth-gears than many men of his age—and he might mention that he was formerly the second engineer for the Brown and Sharpe Manufacturing Company in the States, where, it might be guessed, he had had a great experience in tooth wheels—he could honestly say that the paper of Professor Smith, and the statements which he made in the paper with reference to the curves of his teeth, and the values which he obtained from them, were the most valuable information which it had ever been his pleasure to gather.

Professor D. S. Capper said that, with Mr. Sharp, he might say that he had come to the meeting to learn and not to teach. Whenever the author spoke or wrote, he always said something which worried the brains of engineers for a considerable time afterwards. He was sometimes not quite easy to under-

stand, to begin with, but one found that the deeper one went the more information one got. He was in the condition that he had just had his first look at the paper, and that meant he was in the position of some bewilderment, but he hoped that in the future that state would develop into mature and perfect admiration. He was entirely convinced by what the author had said, and what Mr. Humphris had said, of the fact that engineers could improve enormously in the matter of the design of wheel teeth. He should like, as he was sometimes accused of being a theoretical man, to speak upon the practical side of the question. It seemed to him that at the root of the whole matter lay the question, "With what degree of accuracy could any form of tooth be made which, theoretically, was the right one; or how nearly could they in practice arrive at a theoretical form of tooth?" It seemed to him that the theory had gone far too much on the assumption that to carry mathematical accuracy to its logical conclusion was the sole aim of investigation; whereas the real aim was to see how far a form of tooth could be evolved which would meet the requirements of the practical man. The point made in one particularly pregnant passage in the paper, where the author stated that exactness of pitch was more important that correctness of form, was one which was so constantly overlooked, that really one was almost tempted to say that that was the pith of the whole paper.

With regard to what Mr. Humphris had said as to improved material, he (the speaker) wanted to know how they were going to get accuracy of pitch with that improved material, because it was well known that, do what they would with hardened steel wheels—they might make the pitch and the shape as perfect as they liked—if they had afterwards to harden the wheel the shape was not the same afterwards. He knew that grinding could be resorted to, but that added enormously to the cost.

It seemed to him that no amount of care and skill would be wasted which would bring to them the transcendent achievement that they could get a gear which could be accurately and cheaply made. That was the essence of the whole thing. It was no good paying 50*l.* for a small gear to be fitted to a 10*l.* machine.

Mr. R. W. A. Brewer said he did not think that the waste and wear had been much touched upon. It was very evident that the use of gear wheels was most important in motor-car work—in fact, the most important and the most unsatisfactory part of a motor car. Professor Capper had pointed out that it was necessary that a gear, whilst being good, should be cheap and light. A point which had not been enlarged upon was the silence of gears, and in a motor gear it was very easy to discover whether the gear was going to be silent or not. He thought that it was a most important point that the number of teeth

which came into contact per unit of time should be kept low. One noticed, with regard to motor 'buses, for instance, and generally with regard to the productions of the De Dion firm, where the teeth ran at a very high peripheral speed, that the noise was most objectionable. In cases where several kinds of gear were in mesh at one time, if there were any back-lash between the teeth, there were two or more notes set up, which were at the root of the noise trouble in gear-boxes. With an engine running at a thousand revolutions a minute, and 24 teeth on the primary shaft gearing into 48 on the secondary, two notes were obtained, namely, the middle G, which corresponded to 400 teeth-contacts per second, and the lower C on the middle octave, which corresponded, roughly, to 250 teeth contacts per second, when 30 teeth on the secondary shaft gear into 30 teeth on the driven shaft. Those notes were reasonably low, but, with higher engine speeds than a thousand revolutions a minute, and with larger wheels, if the gear were not very carefully designed, objectionable notes would be produced. Many devices had been brought out in order to reduce the note; but the note still remained, and he thought that it ought to be the object of designers to keep down the number of teeth per unit of time which came into contact. He had brought some gears which would show one or two rather interesting points. One was a point which he had noticed with regard to the gears which were in use on the Central London Railway and on tramways, namely, the very clear marking on the face of the teeth which indicated the positions of the teeth when the next tooth came into contact. In one particular set of gears it could be seen, by careful examination, that the tooth came into contact at certain positions on the tooth face, and those positions were marked, because the teeth were polished. On the single reduction gear on the Central London Railway there would be seen three marks across the tooth faces, which represented the positions of the tooth when the next following tooth came into gear. That clearly showed the hammering action which took place, and which produced the noise. The gears shown were made some four years ago. The distances travelled which he was going to quote represented only the car distances; they did not represent the work which had been done by the teeth. The top-speed pinion teeth had run 45,000 miles, and the tool marks were still on them. The sliding train had run about 40,000 miles, and the top shaft had run 25,000 miles. The large set of gear wheels on the end of the shaft had run continually in mesh with the top-speed pinion gear wheels shown. Those teeth had been continually in mesh with one another. The result was that there was no sign of wear upon them. The wheels were almost of equal diameter. His point was, that those that had

not been continually in mesh, viz. the next train of wheels, showed considerable signs of wear, and the smaller pinion showed more wear still. The sum of the total work on the sliding train was borne by the wheels continually in mesh, yet these did not show any signs of wear at all. He attributed that fact to the vibrations which had been set up in the small shaft which had thrown the teeth out of their true pitch from time to time. There was another point in connection with the shaft. The wear that had taken place there, was due to a hammering action. As far as he could see, it was not due to any sliding of the teeth one upon another. Those teeth had failed by the faces of the teeth crushing. He had run shafts where the teeth had shown another form of failure due to sliding, and the wear had taken place until the teeth themselves were as thin as a sixpence. What happened when they did finally fail was that all the teeth bent right over. No tooth broke. One speaker had pointed out the great waste of power which took place owing to the oil being squeezed out between the teeth which came into gear at high rates of revolution. He was at the present time designing a machine for testing that, and he hoped that, when the machine was fixed and working, he should test a great number of gears and a great number of differences of gear ratio, and he hoped to get some satisfactory results.

Mr. C. T. Alfred Hanssen said his experience had been chiefly with heavy gearing, such as is used for driving pumps from the older type of beam engine, for brick and peat machinery, etc. In such gearing, which transmits considerable power at low speed, the forces on the teeth are large, and it was therefore necessary to make the face of the wheels very wide in order to get sufficient strength. In such gearing he had found one of the greatest difficulties was to get the shafts quite parallel, so as to avoid unequal wear on the teeth; and, however much care was taken during the erection of the machinery to get the shafts and wheels to run true, he had always found that they did not remain so very long, but that the teeth would wear unequally, either on one side or on the other. In gearing for motor cars this probably was less noticeable, just because the shafts were mounted in a rigid iron or steel box, and therefore could be made almost absolutely parallel, but also because the face of the wheels was very narrow.

He therefore thought the author's investigations of great importance, as it would enable engineers to design stronger teeth, and therefore make the face of heavy gearing much narrower than they had been formerly; and if at the same time the noise of the gearing could be reduced, he thought that engineers would owe a debt of gratitude to the author.

Mr. Henry Hutchings said that the last speaker had spoken about the gearing of shafts not parallel and teeth not running true together, very often giving only a point contact. It was a matter of impossibility to get a pinion and a bevel wheel absolutely theoretically correct, however carefully they were cut. They never worked true, on account of the bearings having at least a thousandth slackness. Spur wheels were practically only line contact on the teeth with the ordinary shape. With regard to Mr. Humphris' circular tooth, it could be proved that it gave an absolutely perfect motion, so that there was a surface of contact the whole time. The slip was considerably reduced with a circular tooth in a circular hole. For instance, with a 6-tooth pinion on the involute system, it was practically impossible for it to be made to work. Compared with a circular tooth, it had 90 per cent. of actual slip. With the circular tooth pinion working into about 40 or 50 holes, the slip would be reduced about 20 or 25 per cent. If any one had not heard of the system before, it would pay him to give Mr. Humphris a visit, and see the actual tooth itself. A car before the gear was put in did only about 35 miles upon the level road, but it would now do over 50, which was a fair, actual, practical proof that the amount of friction per pinion revolution was reduced considerably.

Mr. E. G. Beaumont said that the paper was one which certainly required very careful reading if the reader was to appreciate it properly. He would like to ask what improvement was to be expected from gears made to finer limits of accuracy than those made upon the present well-known methods. Some had said that the gears as now made admittedly wore well. That he knew from observation and from experience. Others remarked that under certain well-known conditions they were silent. One of the speakers had referred to one well-known cause of gear-noise, and that was the high rate of make and break of contact of teeth. If the teeth wore well and were silent, or so nearly silent as to satisfy the requirements, the question remained as to how much improvement could be effected in the efficiency of transmission. He did not know whether the author or Mr. Humphris had made experiments to ascertain what the efficiency of their gears was, or how far they were able to show an improvement, if any, over gears made to the well known Brown and Sharpe form, for instance. There had been references to gears made to fine limits of accuracy and showing velocity variations that were extremely small. He would venture to suggest with regard to wheels made to these finest limits, that the oil film between the teeth would equalise the irregularities, and did, in fact, do so. The gears to which almost everyone in the discussion had made reference,

namely, those very fine gears used for motor car transmission gearing, ran, under the best conditions, with copious lubrication, certainly a minimum of 10,000 miles, when the wheels were well carried on properly supported spindles, and after that term of service they showed no appreciable signs of wear; they were merely polished. That suggested that the loss in transmission at the wheels themselves must be very small. There had been some tests made which went to show that the loss in spur-gearing was remarkably small, and from considerations of cost alone, modification which involved machinery that must be of much greater accuracy to be capable of turning out work that was much more accurate than that which now obtained, was not likely to become general. It had been remarked that the area of contact for certain teeth was greater than in others, and that, with some forms of gear, instead of there being merely line contact there was surface contact; but he thought that with gears as now made and used, the question of area contact was of less importance than duration of contact. He would refer, for instance, to the behaviour of modern worm gearing as used for light fast vehicles and heavy industrial vehicles. Many present knew what the formation of thoroughly well made worm gears now was, and although there was only line contact, or little more than line contact, at very high rubbing speed, yet, as long as the lubricant remained in the gear there was no appreciable wearing, but if the lubricant failed the gear was soon ruined. That was an objection which could be applied against worm gearing as compared with spur gearing. Mr. Strachan had referred to gearing made to very fine limits of accuracy, he believed ·0015 of an inch. He (the speaker) thought that in quoting that in connection with epicyclic gearing he was giving the strongest objection to that form of gear. If that extremely fine degree of accuracy were required, and required to be maintained in service, it was time to use a different type of gear which could be used conveniently without the necessity of working to those fine limits, because, with wheels made as nicely as they now were, and running as well as they did when the parts were new, it became more important to consider how the spindles might be stiffened, or strengthened, or made of better form, and how bearings might be made more durable. He thought that when attention was given to those points it would be time to consider the infinitesimal improvements that might be made. As had been already stated accuracy of pitch was of primary importance, and very precise formation of the tooth face of secondary importance. He would like to censure Mr. Humphris mildly for his reference to the modern motor car as a box of mystery. He did not agree with him. It was not a box of mystery, nor was it a snorting, growling leviathan. It was a

question of how the thing was used and how it was maintained. They did certainly meet with examples which fitted the description which he gave to them, but he thought that Mr. Humphris should extend a little sympathy to them as ill-used things.

Correspondence.

Mr. C. H. Wingfield wrote, saying he did not think Professor Smith had explained the application of his system sufficiently clearly to enable *anyone* to design wheel teeth on his principles without a great deal of trouble. As far as he could see, the paper did not take a specific case such as the design of a set of 4-inch pitch-teeth, and explain each step in succession.

Mr. A. Marsden wrote, saying that there was a point on which he wished to ask the author's opinion. This was nearly reached during the discussion, when the involute and cycloidal curves were mentioned. Mr. Brewer exhibited a set of motor car gear-wheels which had run a considerable number of miles without appreciable wear, and had pointed out that the pair of wheels always in mesh were less worn than those only occasionally in mesh. The pressure on the "always in mesh" pair depends upon whether they are used as a first reduction or second reduction, i.e. whether they are situated at the front or back of the gear-box. What he (Mr. Marsden) wished to point out was that the "always in mesh" pair were situated quite close to a bearing, and consequently were in the best position to transmit the power they have been designed for; practically the only factors to cause trouble being the wear on the bearings and the wear on the teeth.

With the sliding gears the case was different, as the sleeve carrying the gear was generally somewhere along the shaft and unsupported by any bearing nearer than those in the ends of the gear-box, so that even supposing the wheel centres could be obtained with tolerable accuracy, it was a state of things which did not prevail very long in use. Taking the "always in mesh" pair. The pressure during approach of the teeth caused the bearings to wear, and the pitch circles to be out of contact by the amount of wear of the two bearings. In the sliding gear the same thing applied, with the addition that at the fixed reduction end the distance was further increased by the wear between the spigot end of the shaft and the socket bearing inside the fixed reduction pinion. In addition to this there was the wear between the sleeve and its shaft also, so that what may have been an initially good mounting, tended to become otherwise owing to the shafts becoming out of parallel as well as further apart.

It seems, therefore, that despite the greater obliquity of thrust in involute as compared with cycloidal forms of teeth, the involute was the only form possible under the circumstances. When it is remembered how accurate the centres of "cycloidal" wheels must be kept in order to avoid irregular angular velocity, and consequently noise, and compare this with the amount of wear which may be allowed with involute forms, the balance certainly seemed to be with the latter.

Taking wheels of say 0·5 inch pitch, and assuming that the bearings have worn owing to obliquity of thrust until their centres were $\frac{1}{16}$ inch or 0·062 inch further apart. The pitch circles of the fixed reduction would thus be this amount out of "contact," and, as the spigot bearing of the shaft carrying the sliding sleeve would probably have worn an amount equal to the end bearings, the distance apart of pitch circles in sliding gear nearest this end would be ·093 inch, which reduced the depth in mesh quite a large amount as compared with the pitch. This was where the involute form scored over the cycloidal form in small pitches for motor car work. With large pitches of say 3 inches, the above wear would be almost negligible. As strain must be co-existent with stress, there was a very cogent reason for the different lines of wear mentioned by one speaker, and also the "makes and breaks" mentioned by another. One had only to imagine the teeth to be made of rubber in order to have a good idea of the difference in pitch between a pair of teeth engaging and a pair just going to engage, the tendency being for the pitch to decrease between a driver tooth and a "just-going-to-drive tooth" on the driving wheel, while the pitch tended to increase in the similar position of the driven wheel, and as the load was distributed according to the number of contacts, the alterations in strain consequent on this could often be traced in gear wheels.

In view, therefore, of the admitted disadvantages which heavily stressed small pitches labour under, it would be very instructive if Professor Smith would give his opinion on how far out of pitch circle contact wheels with his improved tooth forms might be, without being unduly noisy and inefficient. Necessity for very great accuracy may make smaller obliquity of thrust when new, too costly for commercial purposes in small wheels.

Reply.

Professor H. Smith, in reply, said that there was one point which he forgot to put down in the paper. He referred to his belief in the utility of very large fillets at the roots of the teeth, and in large bottom clearance. He believed in a short tooth

above the pitch line, but not in a very short tooth below the pitch line. The large bottom clearance gave an opportunity for a very large radius of fillet connecting the root to the bottom of the gap. If full use was made of this opportunity it enormously increased the strength of the tooth. He quite agreed with what Mr. Humphris had said about the improvement of the material being one great cause of the improvement that had taken place in gearing during recent years, and he certainly thought that it was an omission that he had made no reference to this in the paper. The better material not only gave a greater strength, but it gave also reduced weight. He agreed again with Mr. Humphris that there was a great deal more to be considered with regard to the detail of the design of tooth-wheels than he had been able to deal with in this paper for want of space and because the aim of the paper was restricted to one aspect only of the manufacture of gearing. He wished to confess to some partial sympathy with a remark made by Mr. Archibald Sharp to the effect that the shape of the tooth did not matter very much. Without going that length, he had devoted a considerable portion of the paper to expressing his opinion that the exact pitching from tooth to tooth was more important than the correct shaping of the tooth. If engineers were at liberty to choose any shape of path of contact, the results would not be greatly different, but for different numbers of teeth they must keep to one path of contact. He could show, by tracings, the enormous difference between shapes that did not look to the eye to be very greatly different. With regard to what Mr. Sharp had said with reference to involute teeth being derived from the near crossing portions of double-looped contact-paths, he would say that, if Mr. Sharp would read Mr. Edward Sang's book, he would find quite a large number of different double-looped curves, called by Mr. Sang hour-glass curves, kemends, and modified hour-glass curves, and so on, discussed, and their possibilities investigated. Mr. Sharp had said that the angle of the straight line path of contact of the involute might be anything or nothing up to 15 degrees; but there he forgot one point. He forgot Brown and Sharpe, and he forgot the evil which had been accomplished in this particular section of engineering by over-standardisation. The difficulty in getting any improvement in the shape of teeth introduced lay in the fact that the Brown and Sharpe teeth were used all over the world, and nobody could be persuaded to use anything else, simply because Brown and Sharpe had set the standard all over the world, and were prepared to send out at a moment's notice duplicates of any wheel that had been broken. In this standard form of involute, there was no variation of angle, it was a very

much too large angle, in his own opinion—which fact meant a great deal of stress on the bearings. Mr. Sharp had referred to what he called circular arc teeth. This was an outline made by one sweep of a circular arc covering both face and flank. That led him to admit, that the present was the third time in his life in which he had gone through the subject from the root up to as high a branch as he could reach at the time of each successive investigation. At one time, about 30 years ago, perhaps, he was greatly taken with Mr. Sharp's idea of using one circular arc from top to flank, because, as he had tested for himself, it did give a very small variation of velocity ratio; but it was perfectly evident that, if that class of tooth were adopted, there must be a convex surface working against a convex surface, and the whole gist of the present new proposal was always to get a convex surface working into a concave surface with the difference of the two radii of curvature of those two surfaces as small as possible. Of course, the working of a convex surface against a convex surface was a direct negation of that idea. Again, it was very fascinating indeed to try what could be done with two circular arcs, one sweeping out the face and going a little below the pitch line, and another serving for the flank. The face arc needed to go very little below the pitch line, and another concave surface could be fitted in with a fairly small deviation from the uniform ratio of velocity, but then, again, the working of a convex surface against a convex surface could not be avoided in passing the shoulder contact. That was an impossibility, if only two arcs were used, one for the face, and one for the flank. As the contact at the shoulder was the worst place during the whole contact for friction and wear, he determined to throw that idea aside also. He congratulated Mr. Strachan particularly on his phrase, "makes and breaks," a phrase meaning the hammer blows against which he (the author) protested very strongly in his paper, and which, he said, were much more deleterious to the working of gearing than any fine regard for shape. Mr. Brewer had made some extremely interesting remarks with regard to the testing of the gearing in motor cars, and had mentioned the lower C sound being produced along with a G. He (the author) thought that was a peculiarly disagreeable dissonance. A twenty-five thousand mile run with no more wear than was perceptible upon the wheel produced was a most remarkable performance. He thought, from the look of the wheel, that it was due very largely to the material of which the wheel was made. He agreed very much with Mr. Hanssen upon the futility of getting good shapes of teeth and accurate pitching if they did not have the two shafts parallel, because want of such parallelism threw everything wrong; and it was

frequently the cause of error and comparative failure. He (the author) did not believe in wide teeth at all. He thought that the moderately wide tooth was just as strong under any circumstances as a very wide tooth. He agreed with the statement that one very important feature in any proposed new design was increase of strength. He did not point it out particularly in reading the paper; but the printed paper contained the statement that his design gave a considerably greater strength than the cycloidal forms gave. Of course, if an extravagantly inclined involute tooth was used, still greater strength could be obtained. He wished finally to emphasise one thing which he had inadvertently omitted to state in his paper, namely, the fact that Mr. T. Oldham had assisted him not only in connection with the drawings, but also in the calculations, which were very laborious.

In reply to Mr. C. H. Wingfield's communication, drawings of specific cases (reduced photographically from drawings for 4-inch peripheral pitch) are given in the paper, and the numerical data for many other specific cases are given in the Table on page 107, Appendix V.

In Mr. A. Marsden's communication there is raised the question of how much the gearing fails in respect of uniformity of velocity-ratio when the distance between centres is inexact from wear at the bearings or from bad fitting. This point has been discussed at some length by several of the speakers. The author will endeavour to make an exact calculation of this matter, and, if successful, will send a note upon it to the Society later on. Meantime he may point out that the deviation referred to must certainly be very small, and also that, as has been generally agreed to in the discussion, minute deviations from strictly uniform velocity-ratio are not really of so great practical importance as are smooth, quiet running, good form in respect of friction and abrasion, and strength.

The author desires to express his regret that, owing to an inadvertance, an incorrect and superseded copy of the diagram, Fig. 9, was sent to the printers. The formulæ of Appendix V., Nos. (1), (2), (3), (4), (10), (11), (14) and (15), which give the dimensions shown in Fig. 9 are, however, correct, as also are the numerical examples given in the Table on page 107.

PLATE I.

THE DESIGN, AND WASTE AND WEAR OF WHEEL TEETH

R. H. SMITH.

Fig. 2

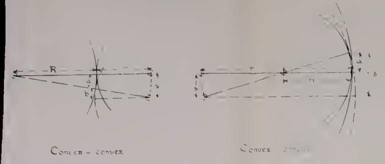

Fig. 1.

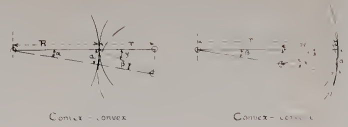

Fig. 3.

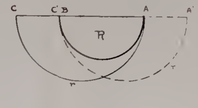

Fig. 4

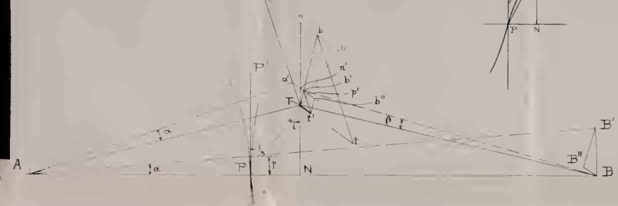

Fig. 5

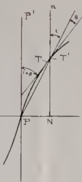

Fig. 6.

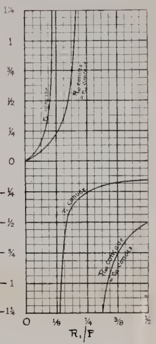

Fig. 7

Angle - Relations for

Flank and Face of Tooth

with ι, exaggerated twice

and ι and β largely exaggerated

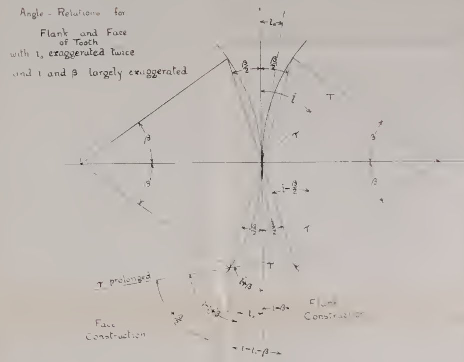

Fig. 8

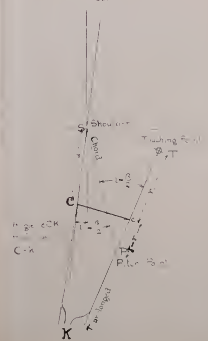

Fig. 9

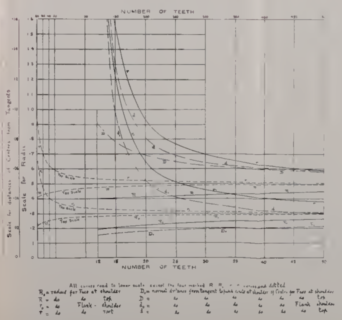

Fig. 10

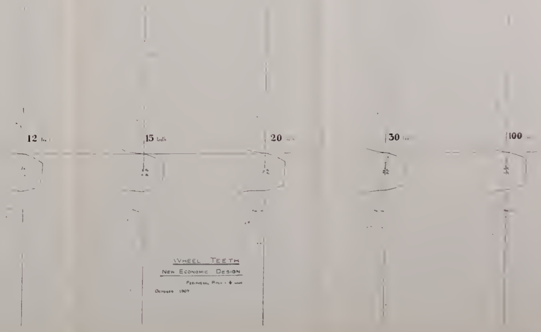

PLATE II.

THE DESIGN, AND WASTE AND WEAR OF WHEEL TEETH.

R. H. SMITH.

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CIVIL ENGINEERS

June 1, 1908.

JOSEPH WILLIAM WILSON, PRESIDENT, IN THE CHAIR.

THE ENGINEERING PROS AND CONS OF THE METRIC SYSTEM.

By ARTHUR H. ALLEN, A.M.I.E.E.

WHAT is the Metric System? Why should we adopt it? What are the arguments against its adoption?

Before discussing the merits or defects of the Metric System, it will be necessary to outline very briefly its origin and nature. It is unique amongst systems of measurement; for all others came into existence in a more or less haphazard fashion, the units possessing no interdependence amongst themselves, and being based upon such purely arbitrary standards as, for instance, the length of a Royal arm, or the average of ten men's feet, whereas the metric system alone was deliberately drawn up with a view to combining all the advantages of existing systems with others that not one of them possessed, and was introduced into general use by the action of Government, under the advice of scientists.

The system was devised at the instance of the French Government—

(1) To take the place of the then existing chaos of weights and measures in France;

(2) To furnish an international system which should be least likely to provoke national jealousy and opposition;

(3) To provide standards which could be replaced from Nature if accidentally destroyed or lost;

(4) To provide a homogeneous system which should to the utmost degree facilitate commercial and industrial transactions throughout the world.

Great difficulty was experienced in securing the adoption of the system in France—naturally, because there did not then exist the inducement which now obtains, namely, that other

nations were already using it, and that therefore its adoption would facilitate commerce. On the contrary, not only was the system new and untried by any nation, but it necessarily interfered to some extent with international commerce. Nevertheless, success was attained; the system was adopted, in spite of all obstacles both internal and external, a fact which counts strongly in its favour; and it is now used throughout France.

The object mentioned under the second heading could only be attained, it was thought, by avoiding the use of any existing unit, and making the values of the new units entirely different from any existing measures. This was, perhaps, an unfortunate decision, as it rendered the change so much the more difficult. There are, however, strong arguments in favour of the course taken, and not the least is the fact that the desired object was completely attained; there has been no opposition of any consequence which can be ascribed to international jealousy.

As regards the third heading, it will be remembered that the original intention was to make the metre, the fundamental unit of the system, exactly $\frac{1}{10000000}$th part of the quadrant of the meridian that passes through Paris, and for this purpose a special survey was carried out. Unfortunately, it has since been found that the earth-quadrant differs slightly in length from the value then determined, so that the metre is incorrect to the extent of about $\frac{1}{4000}$th; however, there was no real necessity to make the metre an exact sub-multiple of the length of the arc, and now that numerous copies of which the exact length is known in terms of the metre are in existence in every civilised country, the metre could be reproduced with certainty from these copies, and the risk of its destruction is exceedingly remote. It is curious to note that the most accurate and permanent natural standard that could have been chosen, and one, moreover, which is the same at every point of the earth's surface, and probably throughout the universe, is the wave-length of light, which is of the order $\frac{1}{1000000}$th of a metre—almost as much smaller than the metre as the earth-quadrant is greater! The value of the metre in terms of the wave-length of the red radiation of incandescent cadmium, which has recently been re-determined with the utmost refinements of accuracy, is 1,553,164·13; it is thus known to one part in a hundred million, an astonishing degree of accuracy, far surpassing that attainable with the best micrometer microscopes. This value agrees within one part in ten million with a determination made in 1893. It will be seen, therefore, that the error of the metre with regard to the arc of the earth-quadrant is of no consequence whatever; a properly qualified physicist, marooned on an island in the middle of the Pacific Ocean, and equipped with all the necessary apparatus

but without a single known unit of measurement, could in time reproduce the metre with what may be regarded as absolute certainty and accuracy.

Measurements such as that cited above are being continually carried on by the International Bureau of Weights and Measures of Paris, which was established with the object of prosecuting researches of this kind, and of propagating the use of the Metric System throughout the world. In this connection it is somewhat humiliating to observe that if our standard yard were destroyed it would be to the International Bureau that we should have to turn for the means of accurately reproducing it —or at least to the results established by that Bureau—for the measurements made under its aegis are far superior in accuracy to those of any similar authority.

The fourth heading opens up more controversial aspects of the subject. It is claimed that this object—the provision of a universal and homogeneous system of units and measurement—has been attained in the main. The system is undoubtedly homogeneous; the extreme simplicity of the decimal notation, and the organised interdependence of the units of length, surface, volume and mass render the system unique. It has been adopted by every civilised nation except the British Empire, the United States, and Russia. Denmark is one of the last to adopt it; by the year 1910 it will be compulsory in that country. Canada has joined the Metric Convention, and New Zealand has passed an ordinance prescribing the exclusive use of the system after a date which, unfortunately, has not yet been definitely fixed. If we adopt the system it is certain that the United States will follow suit, and *vice versâ*. The system is already recognised in the United States so far as pharmaceutical measurements are concerned, and the British Pharmacopœia gives data in both English and metric units. All scientific men use the Metric System, and its use in chemistry is practically universal; many trades, also, in which measurements of precision are necessary, employ the system. Several British firms have adopted it wholly or partially for engineering work, especially those which have a foreign trade, and the system is taught in all schools under Government inspection. The British Colonies have declared in favour of its adoption, but are waiting for us to set the example; and a very large number of our Chambers of Commerce, municipalities and other public bodies have urged its introduction. In 1895 a Select Committee strongly recommended the adoption of the Metric System, to be made compulsory within two years; and in 1904 a similar proposal passed the House of Lords, but, like its predecessors, it was dropped. It was brought forward again in 1907, in the House of Commons,

and defeated by a small majority. It is worthy of note that on every occasion when the subject has been inquired into by a Parliamentary Commission, the report has been favourable to the adoption of the system, and a very large proportion of the Members of Parliament are avowed supporters of it. An International Association for the promotion of a uniform decimal system of measures has been in existence since 1856; the work is carried on nowadays by the Decimal Association, of which Mr. E. Johnson is Hon. Secretary, and to which the author is indebted for the Chart exhibited.

Before dealing in detail with some of the arguments for and against the compulsory adoption of the system in this country, it will be advisable to outline, as briefly as possible, the leading features of the system.

The unit upon which the whole structure is founded is the metre, which is equal to 1·0936 yard. From this are derived multiples—the dekametre (=10 m.), the hectometre (=100 m.), and the kilometre (=1000 m.)—of which only the kilometre is of importance as a rule, though the other units are employed in certain arts and sciences (the dekametre by surveyors, for instance, equals about two rods, and the hectometre by artillerists, equals $\frac{1}{16}$ mile or $\frac{1}{2}$ furlong). The kilometre equals nearly $\frac{5}{8}$ mile. Smaller units than the metre are decimal submultiples—the decimetre (=$\frac{1}{10}$ m.), centimetre (=$\frac{1}{100}$ m.), and millimetre (=$\frac{1}{1000}$ m.)—of which the decimetre is not much used. The centimetre is 0·3937 inch, or about $\frac{2}{5}$; the millimetre is about $\frac{1}{25}$ inch, or $\frac{4}{100}$; and half a millimetre is about the smallest measure that can be conveniently used by eye, though under suitable conditions tenths of a millimetre can be estimated, as in using the slide-rule. The millimetre is the most convenient unit for use in engineering drawings in connection with manufactures.

The unit of surface is the square metre, equal to 1·196 or about $1\frac{1}{5}$ sq. yards. The square centimetre, $\frac{1}{10000}$ sq. metre, is the most used sub-multiple, and the square kilometre, 1,000,000 sq. metres, or 0·3861 sq. mile, the most important multiple; the are is 100 sq. metres, and the hectare (=100 ares = about $2\frac{1}{2}$ acres, actually 2·471) is also a useful unit.

The unit of volume is the cubic metre, equal to 1·308 cubic yards, and the cubic centimetre, $\frac{1}{1000000}$th of a cubic metre, is the sub-multiple in common use (=0·06113 cubic inch) For fluid measure, the cubic decimetre or litre is employed (=1000 cubic cm. =1·761 pint, or about $1\frac{3}{4}$).

The unit of mass is the gram, the mass of 1 cubic cm. of water at maximum density, with its multiple the kilogram, the mass of 1 litre of water (=1000 gm., or 2·2046 lb.). Other

multiples in common use are the tonne (=1000 kg.), almost exactly equal to 1 British ton (0·98), and the quintal, or 100 kg. (= nearly 2 cwt.). Sub-multiples are the decigram (0·1 gm.), the centigram (0·01 gm.), and the milligram (0·001 gm.), used mainly in laboratory work.

The litre is commonly divided into halves and quarters, corresponding roughly to our pint and half-pint, and the kilogram is also divided into the half-kilogram, about 1 lb., and the quarter-kilogram, or ½ lb. The term "kilo" is very commonly applied to the kilogram, improperly, of course, but one cannot control the "vulgar tongue."

The simple relations between the various units and derived units render computations with the Metric System a matter of the greatest ease. Where we would write, say, 27 tons, 6 cwt. 1 lb. 7 oz., the user of the Metric System can, if he likes, write, say, 27 tonnes, 298 kg. 746 gm. When we want to calculate the value of the goods referred to, at so much per lb., or per cwt., or per any of the units named, we have first to reduce the total quantity to one denomination, multiplying or dividing by such awkward numbers as 112 or 16—20 happens to be less inconvenient, because it approaches the decimal system. But the metric computer escapes this process altogether. He simply writes down the figures 27298746 gm., or 27298·746 kg., or 27·298746 tonnes—whichever unit he requires. There is no calculation required for reduction. Think what this means! Let us take another example, say 5 miles, 3 furlongs, 27 poles, 4 yards; here the multipliers are 8, 40, 5½ (!). The metric man writes, say, 8 km. 423 m.; and if he wishes to carry out any computation with this quantity, he merely writes 8·423 km., or 8423 m. What a saving of time and labour—and, not less important, of mistakes! As a matter of fact, it is quite superfluous to write down the figures in the extended metric form first quoted above; they are simply kept in the terms of one or other unit, and, therefore, often need not even be rewritten. What useful purpose is served by splitting up a quantity into acres, roods, rods, and yards? Why can it not be left in a single denomination? The answer may be that it is difficult to form a mental picture of, say, 17,900 sq. yards, while it is easy to grasp the idea of 3·7 acres—but if so, why split up the 0·7 into a variety of smaller units? The metric user has no such difficulty; if he thinks in hectares, he says 9·13 hectares; but if he would rather deal with square metres, he says 91,300 sq. metres.

Again, the simple relation between the units of length, volume, and mass is of great value. Water is universally employed as the standard medium to which the densities of solid

bodies are referred; hence the importance of the fact that a cubic decimetre of water weighs one kilogram, or one cubic centimetre weighs one gram. Given a table of specific gravities, the user of the metric system simply multiplies the volume of a given substance by the specific gravity, and obtains the mass in one operation. We, on the other hand, have first to select the unit of volume, then to look up the density of the substance, and to multiply the volume by the density and the product by the mass of the unit volume of water. This involves twice the labour, with far greater liability to mistakes. To reduce the labour, we find in our so-called pocket-books numerous tables, giving not merely the specific gravity of substances—all that is required on the metric system—but the weight of a cubic foot as well. If our volume is in inches, we have to remember that there are 1728 cubic inches in a cubic foot, and to divide by that awful number. More often than not the volume *is* in inches. But in many cases the weight per cubic foot is in pounds, when we want tons, or ounces, and *vice versâ,* and a further reduction becomes necessary. Again, many things are (in our notation) habitually expressed as regards one dimension in one unit, and as regards another, in another unit. So we find tables for sheet-iron, let us say, giving the weight of 1 sq. foot by 1 inch thick, $\frac{1}{8}$ inch thick, and $\frac{1}{10}$ inch thick; also 1 foot long by 1 inch square, and so on, *ad infinitum.* The metric man is independent of all these refinements; all dimensions are expressed in the same units or in decimal multiples of them, and he needs only one table—the specific gravity table. Even to remember that a cubic foot of water weighs 62·425 lb. requires an effort. It is perhaps easier to remember that a cubic foot of water weighs 1000 oz., and to divide that number by 16 when we want pounds. Unfortunately it is not true; recent determinations (carried out by the International Bureau, a metric body) show that a cubic foot of water weighs 62·288 lb., or 996·6 oz. The difference may not be material, but it exists. Now, in the case of the metric system, it has been proved by most arduous and protracted investigations that whether by good luck or good management, the difference between the mass of a cubic decimetre of water and that of one kilogram is infinitesimal. It is, in fact, smaller, by less than three parts in one hundred thousand. The micron, $\frac{1}{1000000}$ metre, is frequently used to denominate extremely small measurements; otherwise expressed, it is $\frac{1}{1000}$ millimetre, or about $\frac{1}{25000}$ inch. If the decimetre cube of water were increased in dimensions by this insignificant amount (one micron), equal to $\frac{1}{100000}$ of its edge, it would contain slightly more than a kilogram of water (at maximum density and standard pressure). It would be hardly possible to

attain greater accuracy in such a determination, and certainly unnecessary.

Having now discussed the source and characteristics of the Metric System, it remains for us to consider whether, and why, we should adopt it for all purposes in this country, and what disadvantages are alleged in opposition to this course.

The difficulties, the loss of time and useless labour, entailed by the continued use of our existing caricature of a system of weights and measures, have already been incidentally touched upon. These difficulties are met with in almost every walk in life, and are first experienced in early youth, when we are taught to reduce all sorts of compound quantities in a variety of ways, at an immense cost in mental wear and tear, and a serious loss of time. There is so much to be learnt nowadays even to attain to the average level of information, that it is madness to waste valuable time at our most receptive age on hack-work of this kind. It has been argued that the mental training is good for the child, like the study of the dead languages, but are the German and French children less mentally alert than our own? If so, how is it that we are so hard pressed by foreign competition? The drudgery involved in this futile description of mental gymnastics, more akin to the treadmill than to the cricket-field, inspires the child with a distaste for anything in the shape of figures, which often persists throughout his life. He does not even remember the heterogeneous units that he has to juggle with. How many of us can form an idea of the area of a square rod? How many square yards are there in a square rod, pole, or perch (we even give it three names!), or how many in an acre? What is the length of side of a square acre? How many different kinds of gallon are there, or how many quarters? How many grains in an ounce? The British ton is not equivalent to the American ton, as many a trader has learnt to his cost. The only connection between the inch and the ounce is their common derivation, nominally, from the Latin *uncia*, meaning one-twelfth; but the ounce of commerce is one-sixteenth of a pound. The Troy pound is lighter than the Avoirdupois, but the Troy ounce is heavier than the Avoirdupois ounce. If this sort of thing, which could be extended indefinitely, affords good mental training for children, their minds must surely be constituted in a peculiar fashion; we never hear of adults taking up their table-books for a little mental exercise—the only sensation excited by such an occupation is profound disgust.

It may be objected that decimals are difficult; this, however, is not the case. Decimals are simpler than vulgar fractions, and almost as simple as whole numbers. They are not well taught in this country, partly because there is not the same

inducement to teach them as in metric countries, or even in countries where decimal coinage is in vogue. This trouble would very quickly right itself when the Metric System was adopted. The time saved in teaching compound arithmetic and complex units would be far more than sufficient to allow of teaching decimals properly—out of comparison, in fact.

It is not only in childhood, however, that the waste of time and mental effort is incurred; the amount of time and labour spent in office work, due to the use of our units, is enormous, and the opportunities for mistakes unlimited. Where the Metric System has been introduced into British offices it has been found that the employés take to it without any difficulty. Obviously, this clerical work is costly, and unnecessarily increases the cost of our manufactures, thus helping to handicap us in the world's markets. It also increases the cost of goods to us that we purchase from abroad. Further, the openings for dishonest dealing afforded by existing methods of measurement are notorious and inevitable; inspectors of weights and measures have forcibly emphasized this unfortunate fact. There are no fewer than 200 weights in actual use for selling corn; the stone may mean almost anything, and every county has some local measure of its own, the diversity of units being almost incredible. Any attempt to standardise existing units would be a heart-breaking undertaking, and the inspectors confess that they make no attempt to suppress the numerous illegal measures of which they are aware—to do so would be to handicap an inspector's district in competition with others. The only way to secure uniformity is to introduce a system differing entirely from existing units; and this requirement is admirably fulfilled by the Metric System. So well is this understood by business men, that as long ago as the year 1900, forty Chambers of Commerce, twenty-nine Town Councils, eighteen Trades Councils, and numerous other public bodies had passed resolutions in favour of the adoption of the Metric System.

But it is not only in respect of our internal trade that we are at a disadvantage. Our enormous foreign trade, amounting in all to about 1000 millions sterling annually, is carried on very largely, if not mainly, with countries using the Metric System, in spite of the disadvantages entailed by the use of British units. In this connection, there is serious risk in declaring value to foreign Custom Houses; a heavy fine is imposed if a mistake is made in converting from British to metric units, even if the error is in excess, and they will not accept statements in our units. It is not to be expected that foreigners, accustomed for years, or all their lives, to metric units, with their simplicity and convenience, will take the

trouble to learn our complex "system." This applies not only to the Customs, but also to the purchasers, who naturally give the preference to goods measured in units which they understand. In very many cases nowadays, our export houses have to deal direct with the local retailers, to whom our measures are gibberish, and the importance of this matter is increasing yearly; formerly it was customary to deal with agents, who took the trouble and responsibility of changing our units into those of their localities, but modern conditions often render this impossible. Even at the start of transport from our shores, the difficulty is met with; for instance, the sizes of bales are given in inches, and have to be first reduced to cubic inches, then to cubic feet, in order to ascertain the shipping charges, at 40 cubic feet to the ton! Spinning and weaving machinery is an important export, but the trade is greatly impeded by the use of British units. We cannot, as formerly, dictate to our foreign customers what they shall take; we must now provide what they will accept, and our units are to them a foreign language. It is of little use to print catalogues with parallel columns of British and metric units, though that is better than nothing but British. For years our foreign consuls have been urging upon us the pressing need for the adoption of the Metric System, if we wish to retain, let alone increase, our trade.

Incidentally, we may notice the advantages that would ensue from the adoption of a decimal system of coinage, though this is not strictly within the scope of this paper. Many of these advantages are identical with those mentioned in connection with metric computations, such as the absence of troublesome reductions, the simplicity of addition of columns of figures, and the abolition of compound arithmetic. Decimal coinage can be handled with the slide rule and calculating machines, a powerful argument in its favour, and interest calculations lose all their terrors. A long time ago a first step was made towards decimalising our coinage by the introduction of the florin, which was marked "one tenth of one pound." By increasing the number of farthings in 1*l.* from 960 to 1000, the thing is done. We then have the florin equal to 100 farthings, or mils, shilling equal to 50, sixpence equal to 25, and so on. Accounts would be kept in pounds and mils, or in florins and mils, precisely as in dealing with dollars and cents, or francs and centimes. The saving in time and labour would be enormous, and foreigners would have no difficulty with our coinage, neither should we be at sea in foreign lands.

Probably in no branch of industry would the benefits of the metric system be more marked than in engineering works. To begin with, drawings would be figured in millimetres throughout,

and there would be no necessity to mark feet and inches, with the constant liability to error that at present exists through the confusion of ticks with figures. How easily 1′ 1¼″ can be mistaken for 11¼″ or 1′ 11¼″. Or 1 3/16 can become 13/16, and so on. How many errors are made in adding up a series of dimensions containing eighths, sixteenths, and thirty-seconds. How long does it take to find, say, 23/32 on the rule? If calculations have to be made, fractions must often first be reduced to decimals, or voluminous tables consulted. It is sometimes argued that mistakes may easily be made in placing the decimal point; more often than not, no decimal point is required, but if a mistake is made it is so great—tenfold—that the error is generally obvious. For instance, suppose the gauge of a railway were given as 14·3 metres, or 143 metres, instead of 1·43 m. (=4 feet 8½ inches). Who would be misled? Similarly, suppose the length of a rail were stated as 90 m. instead of 9 m.—would the error not be obvious? The liability to error would not be even as great as under present conditions. The ease of taking out quantities, which has been already mentioned, would confer an enormous gain upon draughtsmen.

It might be anticipated that difficulty would be experienced in getting workmen to adopt the metric system. This, however, has been proved by actual experience to be an unfounded belief. Messrs. Willans and Robinson have for years used the metric system for their foreign trade, side by side with British measures, and no difficulty has been experienced; they have adopted the metric system also for their new work—the manufacture of steam turbines. No new tools were necessary on account of the adoption of the metric system. Again, last year the Baldwin Locomotive Works, U.S.A., made 20 locomotives for a French company to metric measures; 19,000 American workmen were affected, and were enthusiastically in favour of the Metric System. The Paris-Orleans Railway Company furnished the drawings, 500 in number, representing 10,000 different parts; the work was to be done in six months, and it was impossible to make new drawings. In a few days six draughtsmen working on the locomotives, and four on the tenders, had got out the shop drawings; the company purchased metric standards, made metric gauges and templets, and supplied the men with metre rules graduated to millimetres. The dimensions were all in millimetres, without any reference to the old system. Not a mistake was made, and the work was satisfactorily accomplished. There was no trouble with the men, and the superintendent of the works, Mr. S. M. Vauclain, was converted to the Metric System.

The limit-gauge system, so extensively employed in engi-

neering works now-a-days, is independent of the system of units; it is immaterial to the workman whether his gauge is exact millimetres or inches—he need not know its dimensions at all. In either case the dimensions are almost certain to be expressed in decimals. Lathes, milling machines, boring machines, etc., need no alteration at all for use with the Metric System; templets are easily altered, if necessary, but how often would any alteration be required? Templets, etc., for existing patterns can be retained for them, and new work made to metric measures. Moreover, all such standard patterns become obsolete in no long time, and if they are not then scrapped, they ought to be! In any case, templets wear out, and have to be replaced. It is easy to alter bushings in jigs. Few measurements, except those where an exact fit is required, are accurately worked to, so that the elaborate calculations sometimes put forward to show the difficulty of expressing British standard dimensions in millimetres are beside the mark; usually the nearest millimetre is sufficiently accurate, and often the nearest centimetre would do. It should be remembered that the same number of significant figures always represents practically the same percentage degree of accuracy. In woodwork carpenters and joiners are not particular to an eighth, and in pattern-making rule-of-thumb allowance for shrinkage would swallow up many millimetres. There need be no trouble at all about standard screw-threads, for these are rarely cut in the ordinary lathe; they are cut with taps and dies which are of fixed dimensions, and it does not matter in the least that they will, no doubt, continue to be cut to inch standards. Owing to the extreme difficulty of cutting accurate threads, screws and nuts are made to gauge rather than by direct measurement. Metric threads can be cut in the lathe with leading screws made to inch measurements. British Association Standard threads, adopted by the Standards Committee, are tabled only in metric units—rare foresight!—but we never hear of any trouble due to the confusion between inches and millimetres. Metric micrometers are conveniently made with one revolution per millimetre, reading to $\frac{1}{100}$ mm.; but inch micrometers have to be made with 40 revolutions to the inch, and 25 divisions to the circle, or some such awkward arrangement involving much mental effort and liability to error. The $\frac{1}{100}$ mm. is a finer and therefore better unit than the $\frac{1}{1000}$ inch, which is $2\frac{1}{2}$ times as coarse. The specifications of the Standards Committee revel in decimal measurements, to many significant figures.

As for engineering calculations, the advantage is so enormously on the side of the Metric System that it is hardly worth discussing. The author's experience in connection with

the design of electrical machinery convinced him, many years ago; it was far easier to convert all the British measurements into metric units, work out the calculations, and convert back again, than to adhere to the British measures. This is indeed a common practice in electrical work. The opinion of Steinmetz, the distinguished American mathematician and engineer, is worth quoting in full. At the commencement of a paper on the "Steam Path of the Turbine," read before the American Society of Mechanical Engineers this year, he says:—

"In the following, the metric system of units will be used throughout, since the incongruous mixture of heterogeneous units, called the English system, requires so many reduction factors, that it can conveniently be used only in a narrow range of mechanical calculations, by memorising reduction factors; but where the investigation extends over several branches of science, as mechanics, thermodynamics, and electrical engineering, the English system is so cumbersome, that it is far simpler to translate the premises into the metric system, carry out the work in the metric system, and translate the result back into the English system, if so desired."

"*If so desired!*" The phrase speaks volumes.

Sir Frederick Bramwell has often been quoted in favour of the existing system, which, he claimed, was the best for mental arithmetic; but he possessed altogether exceptional powers in this respect, and it is a poor argument that, because he was proficient in mental arithmetic, we should put up with the innumerable disadvantages of the British units. Moreover, he mistook the Metric System for the Decimal System. Users of metric units are in no way tied to the use of decimals; they use vulgar fractions whenever these suit their purpose better, and they claim the use of Sir Frederick Bramwell's short cuts as well as their own. On the other hand, Sir Benjamin Baker, an engineer of no less eminence, and of great experience in the use of the Metric System, said that the latter was "incomparably simpler"; the difference was "simply marvellous!"

Attempts have often been made to decimalise our existing system, and there is even an Association for the purpose of promoting this object. All such attempts are doomed to failure, for they would lead to endless confusion. To decimalise the inch would still leave all the disadvantages of the foot and yard. The Armstrong works have used the decimal divisions of the inch for 40 years, and rightly, as they thus get rid of the thirty-seconds and sixty-fourths, with their "bare" and "full" allowances. But to alter the inch, even slightly, would be fatal; the new unit would be too nearly like the old, and endless confusion would result. Germany actually attempted to do this

very thing, but gave it up after inquiry, and adopted the Metric System. There was no friction due to its adoption, nor was there any in Austria. In fact, wherever the system has been introduced, objections have quickly died away, and no country which has adopted the Metric System—since the early struggles of France—has ever given it up. Germany finds no difficulty in manufacturing with these units; if she did, we should find her competition less effective than it is. The objection often advanced, that textile trades, which form so very important an industry in this country, would suffer from the change, has been disproved by facts. Mr. Lloyd-George stated last year that he was informed, on good authority, that cloth manufacturers could quote and deal in metric quantities without any difficulty at all.

Perhaps one of the most serious obstacles to the general introduction of the Metric System would be found in the necessity of replacing all the standard weights of retail traders. Their balances would not be affected, but a new set of weights would cost from 16*s.* to 2*l.*, say 30*s.* on the average. A Government grant might facilitate the conversion.

The decimal system of weights and measures originated in this country. It was devised by James Watt in 1783. Probably his proposals gave the first impetus to the movement which was carried out so successfully in France a few years later. Yet we are amongst the last to take advantage of the benefits to be derived from a rational system. Why should we wait?

We are a conservative nation in this, as in other things; public opinion must favour the conversion, and the people must be educated to realise the advantages to be gained. The question must be kept alive in every possible way; and if this brief sketch of an immense subject—upon which volumes could be written—aids the good work, even to a small extent, by giving rise to a discussion, the author's object will have been achieved.

DISCUSSION.

The **President** said that it was his privilege to propose a vote of thanks to the author for his interesting paper. Although many papers had been written on the subject from time to time, this Society had not yet had one dealing with it, and it was well that they should take up the consideration of it, for this country was in a state of some confusion upon the question. If the facts could be known, it would probably be found that at the time the Tower of Babel was being built people not only had one common language but also one standard of measurement

(though it might not have been the metric system), and that, when their language was confounded, their measurements were confounded too; and had so remained to the present day! He was anxious that the meeting should discuss the subject more especially from a practical and an engineering point of view. It appeared from the paper that there were more "pros" than "cons"! As to some of the arguments which had been brought forward, there could not be any question that it would be much more simple to train children in the metric system than in that which now prevailed amongst us. Indeed it had been calculated that about one-eighth of a child's time at school could be saved in this way. It was some thirty or forty years since the House of Commons first passed a Bill in favour of the metric system, though the measure did not then get any further. Such a matter could not of course be settled by a meeting like the present; but he agreed with the statement of the author that the subject should be kept before the public and that they must be educated in respect to it. The author had touched upon one serious result of such a change, namely, the necessity for providing new sets of weights and measures throughout the kingdom, and had estimated the cost at from 16*s.* to 2*l.* in each instance on the average. He had also suggested that a Government grant should be made to facilitate this conversion. The Chancellor of the Exchequer might be in favour of the metric system, but whether he would be in favour of such a grant was another consideration. Probably, it would have to be a larger one than the author had realised. He hoped that they would hear an expression of opinion from that energetic association, the Incorporated Society of Inspectors of Weights and Measures, and from other leading authorities who were also represented at the meeting. The Society were much indebted to the author for putting very important facts into a small space and stating them in a practical manner.

The vote of thanks was accorded by acclamation.

The two following communications were read by the Secretary :—

1. A letter dated May 18, from Mr. F. R. Simms, Managing Director of the Simms Manufacturing Company, Ltd. :—

"Nothing would have given me more pleasure than to attend the lecture on the Decimal System, and to take part in the discussion, but on June 1 I shall be abroad. I regret my absence all the more since, as you know, I am a very strong advocate of the metric system."

2. A telegram (received 5.30 P.M. June 1) from Mr. Kenyon, of Sheffield :—

"Sorry unable attend meeting Whitehall. Decimalisation

must come. Delay increases expense and the inconvenience of the change. Aggregate cost certainly large. Probably per cent. annual turnover insignificant. Sufficient arithmetical gymnastics in mathematical practical application of natural laws. Universal system desirable. Will adoption British units secure this?"

Professor C. V. Boys, F.R.S., in opening the discussion, said that it seemed impossible to say anything new on the subject either for or against. Nearly all present must feel that, if the country were starting anew, the arguments which had been used in favour of either the metric or a decimal system would prevail, and people would be absolutely all of one mind. The practical difficulties lay in making a change. Not being a business man, he did not feel competent to speak of the subject from the business point of view; and he did not wish to gloss over the difficulties. He wished, however, to refer to the subject in one or two of its minor aspects. There were really two questions that must always be considered separately when the subject was dealt with. One was the use of any decimal system at all, and the other was the use of a particular decimal system, namely, the metric system. Against every decimal system, they were often told, and more especially by those who liked to call themselves "practical," that a decimal division was absolutely unsound, or was, at any rate, a most inconvenient division. It was said that workmen did not understand it, and that they liked to divide by two over and over again. In doing this, they might get as far as the 128th, but then they got "mixed." One heard a great deal about the British inch as a perfect measure in itself, which was not capable of being superseded by any new-fangled measure. There seemed to be some special virtue about the British inch which he was unable to understand, but this was often dwelt on. They heard of the peculiar merits of the thousandth of an inch in connexion with nicety of work, and it was said that workmen could not understand decimalisation, but the thousandth of an inch could not be got by dividing by two, however long they divided; so it was of no use to talk about the advantage of dividing by two for ever, unless they were prepared to give up the thousandth of an inch. Speaking as a constructor who knew what nice work was, he thought that the thousandth of an inch had the disadvantage of not being small enough. Two things which differed by a thousandth of an inch, especially if they were small, differed very badly, and they would shake and rattle; and, therefore, the thousandth of an inch was insufficiently small as a unit. It was good enough if the thing were large, but not otherwise. On the other hand, in the case

of the one-hundredth of a millimetre, where work was produced by the grinding process or by the cutting process, they were just upon the limit of straightforward workshop construction. It seemed, therefore, unnecessary to go as far as the ten-thousandth of an inch for the unit. In saying this, he must not for a moment be supposed to mean that no construction required greater accuracy, because they all knew those magnificent Swedish gauges. The error of a pile of them put together was practically outside the range of the workshop micrometer.

He was mentioning only these things as illustrating the difficulty that he found when they were told that the metric system was of no good in the workshop. He could not think what the difficulty was. He had talked with his friend, Colonel Crompton, who was presently going to put him right, but he had never been able to ascertain anything except that gentleman's assertion that the metric system was not good. He had never understood why it was not so.

It was said that, if they introduced metric nomenclature into a specification or into regulations, they would be liable to introduce difficulty and confusion. He was one of the Metropolitan Gas Referees, and about ten years ago, when the new system of making official tests of London gas was designed, and the official description of them was sent out for use in all the testing places, it was considered that it would save time and trouble, as they were starting *de novo*, to adopt the millimetre as the unit of measurement; and the whole of the detail and all the construction of the photometers as now used were designed at that time on the millimetre basis. There was some curiosity as to the result. The Referees knew quite well that, if the system had a fair trial, there would be no difficulty in understanding the millimetre. In spite of the fact that the *Journal of Gas Lighting* was a paper which took a superior attitude as to the British inch, the only remark that they made at all about all these terrible difficulties was something to this effect: "We notice that the dimensions are given in millimetres." There had been no difficulty in any quarter, and there had been no complaint. Not the slightest objection had been taken whatever. Of course, all chemical operations were carried out, as they must be, by the metric system. The only measure which they could not adapt to the metric system was the cubic foot for gas. The cubic foot was a Parliamentary measurement. But, directly they had got their gas, they left off using the cubic foot, and they dealt with the chemical and the linear measurements on the metric system, and no difficulty had been found.

Colonel R. E. Crompton said that it was interesting that the Society of Engineers had dared to discuss a subject which

in other societies has been tabooed on account of the extreme ardour—to say the least—which was shown in the debates whenever the subject was mentioned. In America, most extraordinary statements were made about the number of men in workshops who had voted in favour of making the metric system compulsory; but there was nothing which made the average American engineer so angry as to tell him that this was the case.

The fact that people varied so widely in their opinions upon the question arose probably from the defects of decimal numeration. Our forefathers started with dozens, grosses, and so on, and some foolish person abandoned those duodecimal divisions and went into the decimal business, and then our ills began. It was the decimal trouble that he found fault with, and not the metric system or the convenient arrangement of interchangeable units which had followed on the introduction of the metric system in France, and which was, undoubtedly, the cause of its wide adoption in countries which were not very well established and in which change was not a costly thing. They were faced with this problem: why did England and America absolutely hang fire? For years both countries had had the choice of two systems. All that time the minority had been howling out to compel the majority to their way of thinking. From the fact that so little progress had been made, it seemed that there must be something in the two countries which made the majority feel that a change was not desirable. There were mechanical reasons why decimal fractions, with their repeaters, did not work in well with any system of dividing inches. People preferred aliquot parts. In textiles, where people had to double and double again, the decimal system again did not fit. For that reason, the English count was still preserved all over the Continent.

His theory as to the reason why people thought so differently upon the subject was that the world was very much divided into two classes. One consisted of the thinkers, the analysts, and chemists, who dealt with matter as it exists, and had to dissect it and consider it, but had not got to construct with it. The other consisted of the engineers who had to deal with constructural design. For screw-cutting, dividing engines, rolling mills, and things of that kind, they found that 12 was a more convenient figure to work to than 10. If they designed in the metric system, they figured the bolts in inches or "Whitworth," which meant English dimensions. There were many anomalies of that kind.

It would be an undoubted advantage if youths could be saved from the trouble of all the old-world tables. The stoutest

supporters of the English inch would admit that. They did not at all want rods, poles, or perches, and they were quite prepared to have an acre defined as so many square yards. But when the author of the paper spoke of altering the mensuration of surveying in England, they must remember the immense confusion and trouble with title deeds and everything of that kind that would follow any change in their ground or land measurement. In Austria and other countries where the metric system had been introduced and made compulsory, they still had to retain their old land measurements. It was the same in many parts of France and Germany.

Why had the metric people never got their system for calculations at sea or for navigation? As a matter of consistency, time ought to be decimalised. Why had it never been done? The fact of the matter was that the changes in such things would be so serious and so confusing, and introduce such difficulties, that the advantages would have to be huge to justify a change. He did not think that any of the advantages claimed in the paper were really very great. Broad statements were made to the effect that the metric system would facilitate trade, but nobody had brought forward the least argument to show how it would do so. If he turned out a steam engine of a certain size from his workshop, what would it matter that it had been designed in inches, although the purchaser might have it listed to him in millimetres? It would make no difference, and the author over and over again admitted that fact. The author referred to Messrs. Willans and Robinson; but that firm had several times publicly disavowed the fact that they had adopted the metric system exclusively in their works. They had always had the two systems alongside one another for many years, just as he (Colonel Crompton) had in his works. The author had stated that they were able to make parts interchangeable under the two systems. Of course, if they made them to the same gauge there was no trouble about that. The whole question of accurate manufacture was practically a question of limit gauges.

The author made a strange statement that elaborate and accurate calculations were not necessary. "Few measurements except those where an exact fit is required, are accurately worked to, so that the elaborate calculations sometimes put forward to show the difficulty of expressing British standard dimensions in millimetres are beside the mark; usually the nearest millimetre is sufficiently accurate, and often the nearest centimetre would do." What sort of trade was the author referring to? If they had to lay down standards of workmanship, the thousandth of an inch, as Professor Boys had said, was a very coarse measure-

ment indeed. He had been for four years on a committee with regard to standardising, and every man who had anything to say with regard to the improvement of workmanship by standardising had admitted that the ten-thousandth of an inch was a convenient measure, and that there was no possibility of getting English mechanical engineers to adopt gauges graduated in millimetres, although, if they were graduated in millimetres, the one-hundredth of a millimetre would be a perfectly easy thing. Then the author said, "Inch micrometers have to be made with 40 revolutions to the inch." He (Colonel Crompton) thought that they could make them nowadays with 100 revolutions to the inch. Both the author and he were electrical engineers. He (the speaker) had worked on the C.G.S. system, and he admitted its convenience for electrical measurements.

The statement about the "bare" and "full" allowances with reference to the Armstrong works was, he thought, rather a libel on Sir Andrew Noble. He did not think that his friend, Mr. Brackenbury, who was on the Standardisation Committee with him, would admit that they had to get rid of "bare" and "full" allowances. Thousands of English firms had been using "mils" for thirty years, and their accuracy had been obtained by using them.

Sir Benjamin Baker was made out to be a strong advocate for the metric system. What he said was that it was no trouble to use metres on the Assouan dam. But he (Colonel Crompton) did not think that the millimetre difficulty arose on the Assouan dam.

The paper was an interesting one, but people would never agree on the subject, and they would waste a good deal of time in talking about it. They might exercise their mental powers by criticising what other people said, but they would never get any farther, and it would take a great deal of conversion to make the mechanical engineers, whether of America or of England, abandon their present system of measuring and weighing. As regarded lineal measurements, there were nearly 100,000*l.* invested in templets and things of that kind which could not be wiped away, and which could not be measured in even fractions of a millimetre.

Major G. Hurlstone Hardy said that with regard to the paper there was one little correction which might be made. James Watt visited and conferred with the philosophers of the French Revolution; hence it might be taken, not as a supposition, but as a fact, that his project of decimalisation, after being tabooed in England, was taken up with that fervour for novelty which then existed in France. A good fighting speech had been made by the last speaker, and he (Major Hardy) had heard many

such speeches from the late Sir Frederick Bramwell and other opponents. He had never thought anything of Sir Frederick Bramwell's argument, but Sir Frederick's personality gave weight to what he said. They must not be overborne by big names, for big people made big mistakes sometimes in their own matters of business as well as outside. In General de Montholon's Memoirs the reasons are mentioned of Napoleon's opposition to the use of the metric system, but they do not now impress one as prescient wisdom. Its use did not immediately follow the French Revolution, for it did not really become compulsory and general till 1840. There was, however, one profound remark of Napoleon the Great which had got overlooked. It has been said that there was no universal measurement in his day; but there was. There was the fathom; 100 fathoms then, as now, made a "cable's length," and 1000 fathoms the nautical mile. This is the measurement which should have been standardised in that day; thus there was a universal decimal system existing at that time, and if the French philosophers had taken the fathom, that would have been a good thing to have standardised. People would then have accepted it universally, and there would not have been any international jealousy. The fathom was a natural thing. It was double the length of the extended arm of a man, and it suited nautical use and geographical use.

He (Major Hardy) would put a well-defined limit on decimalisation; there was no need to alter a great deal of what now existed. They had for land measurement the link and the chain. 100 links = 1 chain. That was a decimal measure in itself. Land measure in England had long been decimalised, and there was no need to disturb it. The popular appeal should be for a universal linear measurement, and now this must be the metric system which was the one that a spirit of conservatism opposed. He thought that there was great advantage and much common-sense in putting a restriction on fantastic decimalisation. The restriction would be for land and sea, geography and time. With such a restriction, Admiralty charts, land measurements, and title-deeds need not be altered. With regard to lineal measurement there was opposition in certain trades, but he thought that the opposition had been magnified. With regard to textiles, it was, he thought, only necessary to have the retail measurement in metres rather than yards over the counter. Cloth and the thickness of the cloth depended on the loom in which the cloth was woven. Then cloth got altered in shrinking and in fulling. Consequently, thickness represented different qualities. Thickness in paper, and in the count of yarns, could be left in the hands of the trades concerned. All

that was mixed up with the quality of the goods or the use of numbers as an index of the fitting size of hats, gloves, boots, etc., should be wiped out from the project of having decimal measurements. Reform was mainly wanted with regard to the linen-draper's yard in commerce ; all else would follow. With regard to engineering, the matter had been very well put. He certainly preferred the decimal system with its decimal point to quarters and dividing in that way. Sir Frederick Bramwell always said that anyone could understand a quarter, but he might not understand 0·25 ; but if so, he (Major Hardy) thought that there was something wrong with the education of the man. Decimals were not well taught in primary schools, and he thought that they ought to be taught in preference to fractions. People would naturally get into fractions as much as required without any teaching at all. When children were properly instructed in the decimal system, they learnt a great deal more than the decimal system. They learnt "percentages," a system of ready-reckoning, and a great deal of useful knowledge which was now very hazy in the ideas of the general public.

The proper limitation of the metric system would wipe out a great many of the objections. Horology did not require any interference whatever. It followed with the geographical mile, and the two fell in very well together. In looking at a clock we took a near inspection of a circular disc, and the division into twelve came very easily and naturally. It suited the custom of having twelve hours in the day. It was a pity, however, that we did not count twenty-four hours after the Italian system. English people did not want, like the French Intellectuals, to go too much into detail and make ten days to the week, or ten divisions on the clock dial. The French have reverted in angular measurement and divide the circle into 360 degrees. Again, the present division of four quarters very well suits the weathercock; they could not put ten divisions to a weather cock on the top of a church steeple. Four points were amply sufficient, but thirty-two better suited the nautical compass. Again, with regard to the old system of semaphore signals. Ten would be an inconvenient number to be viewed from a very remote distance. A count of eight suited the purpose exactly. The positions of the semaphore arm accordingly counted in the commonest and simplest code as, 0, 1, 2, 3, 4, 5, 6, and 7. There were other advisable limitations, and if these limitations were properly considered, they would wipe out three-quarters of the objections raised by the last speaker. Interference with trade would be very little indeed. People could buy wine or beer by the dozen bottles or by the bottle, and legislation would not interfere with them. They could

leave all the present measurements until the bottles were worn out, but the litre would be the lawful standardised measure, whilst old bottles would be understood as having a capacity expressible in decimal parts of the litre.

His own difficulty with regard to the metric system was that they were wrong in trying to introduce decimal measurements before they had decimal coinage. If the matter was to be taken up by those who were enthusiastic, they must, he thought, as a matter of diplomacy, put decimal coinage first. The matter must be run democratically. Poor people had a trouble with the coinage, and they wanted a ready means of reckoning. For instance, they would buy, say, 3 lb. 7 oz. of bacon at $9\frac{1}{2}d.$ lb., and they could not count the amount exactly; the shopman put on a little for his own benefit, and the tiny fractions, repeated over and over again by the hundred, made a source of rather substantial profit to the small tradesman, whilst no buyer knew whether he was paying the right sum or not. He thought that the decimal system of coinage ought to take precedence of the metric system. If both reforms are to come, the ordinary "man in the market" will surely say—let us have them both at once, and it will soon be over. It was said that people would never learn the metric system, but it had been proved over and over again in every country where it had been introduced that the people did wonderfully soon learn to understand and value it. He was a "bit of a Liberal," but he believed that a little compulsion was sometimes wholesome. It was a case where the thinker should think and enforce his view for the general good.

Mr. J. H. Twigg said that irrelevant and trivial objections were often made against the metric system: for instance, it was stated in a lecture before the Royal Society of Arts (December 1906), that "it is an essential part" of the metric system to have 100 degrees instead of 90 in a right angle. That lecturer, Sir C. Watson, R.E., should have known that the metric system relates only to length, surface, volume and mass, so that angles, being mere ratios of arc to radius, do not enter into the system at all, nor do the measures of time, money, or thermometry. The metric system, moreover, does not exclude common fractions, as some people imagine.

Letters have recently appeared in the press regarding the useful work now done for preparing standards of construction and material. In some of this the Centigrade thermometer was used, possibly to give a sort of scientific flavour to the proceedings, but the metric weights and measures are excluded, the work being intended for Great Britain. Now, however, it was announced that the scheme was to be international.

Presumably then the convenient round numbers chosen for British measurements must be translated into awkward fractions for the use of metric Europe. The most widely spread of all objections said nothing against the metric system itself, but declared that the cost of factories, machinery, and industry would be ruinous The simple answer was that no one has ever proposed to force the metric system upon factories or industry of any kind, but only upon the buyers and sellers of goods. Nevertheless, incredible as it might seem, the House of Commons was told the contrary by several speakers in the debate on the Bill of last year, and this was widely believed, though the Bill was supported by the high authority of Armstrong-Whitworth, Vickers-Maxim, Sir W. Mather, Lord Kelvin, etc. In this matter the most serious question for England was, "Should she stand alone while the other nations advance in metric reform?" Till last year it could be said that in Europe perhaps Denmark and Russia might join us in some scheme of British units. That hope, however, vanished last October, when the world was officially informed at the Paris International Conference that Russia is now steadily introducing the metric system, having fully established it in Finland, and that Denmark has completed her arrangements for the same purpose. By these measures about 150 millions of people are affected. As regards Asia, also, an official announcement was made that Japan, with its 40 millions of people, has in hand a law for metric reform. China has an official committee considering the question. The British Free Colonies have urged, with unusual asperity, that England should join them in adopting both the metre and decimal coinage. It was said that British workmen are "wedded to the inch," and particularly to the thousandth part of it.

Why would not 25 millimetres and 2½ hundredths of a millimetre be efficient substitutes? Some schoolmasters condemn the metric system as being so easy that it deprives children of useful exercises in the difficulties of English measures. According to Mr. A. J. Balfour, the proper reply was that we should then abolish high roads and take healthy exercise in jumping ditches!

Mr. Miller, of the Bank of England, has estimated that if we reckoned money decimally, as the metric weights and measures are reckoned, the Customs House alone would save £20,000 a year.

We may have to wait a hundred years for the metric system unless a beginning be made by some sort of compulsion, limited expressly, as above stated, to buying, selling, and, perhaps, the transport of goods. Not the least important of the

official reports at the International Conference of last October announced the great success of this method in Switzerland, whereby the metric system, after being made familiar by compulsion in commerce, has been voluntarily adopted by the people in manufactures.

Mr. D. G. Mackerdy said that he attended the meeting as representing the Society of Inspectors of Weights and Measures. His society still very strongly upheld the introduction of the metric system. About fifteen years ago he had the honour of reading a paper before the Society of Inspectors of Weights and Measures at a meeting in Edinburgh, and since then the matter had been brought up from time to time. His society had actually gone so far as to introduce a Bill into Parliament, but they had been unsuccessful in getting the whole Bill passed into an Act. Unfortunately, the part of the Bill relating to the metric system had, like Jonah, to be thrown overboard, so as to save the main portion of the Act. The Act to which he referred was the Weights and Measures Act of 1904. Inspectors of weights and measures dealt only with the question from the point of view of trade. Reference had been made to the cost which would be brought upon shopkeepers by the change from the imperial to the metric system. That is, as stated by Mr. Allen, one of the greatest obstacles to the introduction of this system. The statement that the change would only affect weights was not entirely correct, for there were such things as spring balances, and there were also automatic weighing machines, and these would require to be completely altered if the metric system were introduced.

He disagreed with the writer of the paper with regard to his statement that the inspectors of weights and measures made no attempt to touch the numerous illegal local measures which were used in different parts of the country. He admitted that in many districts commodities were asked for and sold in terms of customary local measures, but imperial measures were used, as for instance, fruit in the South of Scotland is sold by the Scotch pint = $\frac{1}{2}$-gallon. He could assure the meeting that his society would very much welcome the introduction of the metric system.

Mr. E. J. Silcock said that there was one feature of the case in favour of a decimal system which had not been mentioned : he referred to the introduction of the decimal system in relation to the scales of plans, and particularly of maps. As they all knew, the old Ordnance survey was published to scales of one inch to the mile, six inches to the mile, and five feet to the mile. When the new Ordnance survey was being produced, those scales were revised and scales of $\frac{1}{500}$th and $\frac{1}{2500}$th were

adopted. Those scales have turned out to be exceedingly useful. The whole of the areas which were computed on the $\frac{1}{2500}$th scale were expressed in acres and decimals of an acre to the third place of decimals. Colonel Crompton, in referring to the matter, had said that the decimal system would cause confusion with regard to the areas of land. He (Mr. Silcock) could assure him that no such confusion had arisen. British farmers were probably the most conservative of all conservative Britishers, but they had adopted the decimal system for the acre. If one took up any modern plan of farmlands, he would find the areas of the fields set out in acres and decimals of an acre, and the farmer could deal with the decimals, even if he had never seen them or heard of them on a plan before. The same remark applied with regard to legal documents. In the more formal legal instruments, such as conveyances of land, the scheduled areas were often given both in decimals and in the old measurements, but the total was usually brought out upon the decimal system, and then reduced to a final total of so many acres and so many roods and perches. It might interest the meeting to know that on the scale of $\frac{1}{2500}$th, one square inch was as nearly as possible equal to one acre. Therefore, for all rough purposes, if they wanted to take out the approximate area, by measuring the length and the width in inches they would get the area in acres.

CORRESPONDENCE.

Mr. A. S. E. Ackermann wrote, saying that as one whose work involved a large percentage of calculations, he heartily supported the compulsory adoption of the metric system, and had great difficulty in understanding how any engineers who actually made use of mathematics and calculations in connection with their work could possibly do anything but support the compulsory use of a system so admirable, simple, and labour-saving. The explanation of the opposition seemed to be historical or psychological, for it was, unfortunately, a fact that all reforms had been bitterly opposed by one section or other of the public, and in many cases a section that should have known better. For example, all sorts of wise people told George Stephenson that he could never make a locomotive which could travel at a high speed and pull heavy loads; that the locomotive would devastate the country around the track, kill all the cattle with fright, and that the passengers would not be able to breathe because of the high speed with which they would pass through the air!! Other obstructionists said that the Atlantic could never be crossed by steam power. Thousands of people in England opposed the adoption of the Gregorian

Calendar, and even after it was adopted they cried out "Give us back our eleven days." Others said that submarine cables were absurd, and so forth, and even quite recently we had heard wireless telegraphy pooh-poohed by a well-known engineer (interested in submarine cables!) as commercially impracticable.

With regard to the views of American engineers on the subject, Mr. Ackermann said he wished to refer readers to the "American Machinist" of May 26, 1906, in which it would be seen that the following two resolutions by a Committee of the American Institution of Electrical Engineers on the metric system were submitted to the whole of the members in the United States for ballot by post:—

"That this committee unanimously recommends the introduction of the metric system into general use in the United States at as early a date as possible without undue hardship to the industrial interests involved.

"*Resolved.*—That this committee favours such legislation by Congress as shall secure the adoption of the metric system by each department of the National Government as speedily as may be consistent with the public welfare."

The total number of members invited to vote was 3300, and the result was as follows:—

In favour of the resolution	1569
Against the resolution	178
Total vote	1747

Majority in favour of the metric system, 1391.

The writer stated that he wished to call attention also to a most admirable article on "The Advantages of the Metric System," by George Moores, which appeared in the October 1902 number of the "Empire Review," published by Macmillan and Co. Mr. Moores is now Secretary of the British Weights and Measures Association, and in trying to explain away his somewhat altered views, it was reported that he says there is a time when one swears *by* the metric system, and later on a time when one swears *at* it. This witticism, however, was not sufficient to explain away the admirable arguments used, and still less the actual facts mentioned by Mr. Moores in his article. For example, he says on page 263 of the "Empire Review" that, "in Sunderland a bushel of corn equals 46 lb.; in Shropshire, 72 to 75 lb.; in Hereford, 63 lb.; in Newtown, 80 lb.; in Cornwall, 240 lb. A stone of live meat is 14 lb.; dead meat, 8 lb.; cheese, 16 lb.; glass, 5 lb. A fodder of lead at London and Hull is 19½ cwt., at Newcastle it is 21½ cwt., whilst at Derby it is 22½ cwt. I could give pages of

such incongruities. Why, in weighing corn alone, there are over 200 different weights used, whilst of measures for selling it by measure, there seems to be no end." Mr. Moores also gave the following quotation from the late Sir Benjamin Baker, the eminent engineer: "I am equally familiar with both systems in consequence of having carried out works abroad, and when I return to this country from such work and experience of the metric system, I think there is nothing more foolish in this world than our weights and measures." Other ridiculous examples of the so-called British "system" are as follows:—

There are $437\frac{1}{2}$ grains to 1 ounce avoirdupois. A "drachm" is not equal to a "dram," though the words are pronounced alike. A butt of wine *varies with the kind of wine* (!!), while a gallon contains 277·274 cubic inches.

The metric system was so simple that the whole of the more usual units and their inter-relationship might be learned in a few hours, whereas it was highly doubtful whether any English man or woman had ever lived who knew the whole of the British "system." Mr. Ackermann stated that in March 1907 he had some correspondence with Mr. Moores, Secretary of the British Weights and Measures Association, in which Mr. Moores set him a very simple problem, namely:—

"Given the fact that a cubic decametre is equal to 1,000,000 cubic decimetres, how many litres are there in a barrel of wine?"

To this Mr. Ackermann replied:—

"On looking up the definition of 'barrel' in Lloyd's Ency. Dic., I find, 'In one for holding liquids, the capacity is usually from 30 to 45 gallons,' hence before I can answer your question it will be necessary for you to select a definite number of gallons. Supposing you select the mean of the above values, viz. $37\frac{1}{2}$ gallons = 300 pints, and that you had not omitted in your question to give any connecting link between the metric system and the British $\frac{\text{Muddles}}{\text{Measures}}$, then apparently the problem reduces to $\frac{300}{1\cdot 7607} = 170\cdot 4$ litres.

(By slide-rule, with a probable error of $\frac{1}{10}$ per cent.)

As your question stands, it appears to be of the order 'If a herring and a half cost $1\frac{1}{2}d.$, what will a ton of coals come to?'

"With your knowledge of British weights and measures, perhaps you will *work out* the following problems:—

"(1) If one imperial gallon = 277·274 cubic inches, and 93 gallons of Marsala = 1 pipe, then how many grains are there in a bushel of wheat?

"(2) Given that 62·726 square inches = 1 square link, that 2·295 links = 1 square foot, and that 1 metre = 39·37079 inches, how many square links are there in 1 'are'?"

Though there was a little further correspondence, Mr. Moores never worked out either of the problems! It should be noted in the foregoing problems that the relations requiring three places of decimals to express them are between British units.

In conclusion, the writer wished to call attention to a most excellent pamphlet (price 3*d.*), published by the Decimal Association, of Salisbury House, E.C., entitled "The Metric System; a Reply to the late Mr. Herbert Spencer," by Joseph H. van Biene.

Mr. W. C. Wedekind wrote saying that one speaker had asked why it was that a small minority had achieved such small success in convincing the large majority of the advantages of the metric system. If he would assume that the coast-line of this "tight little island" does *not* confine all the intelligence to which the metric system applies—and one of the greatest aims of the system was to provide an *international* system—the wonder was that the small minority of opponents held back in the face of the immense majority who are quite satisfied that they benefit by the metric system, and who are utterly indifferent to the minority who prefer to ignore the benefits offered by the system advocated by the author of the paper.

If we analysed this minority of opponents, the vast majority were not opponents of the metric system, but were simply ignorant of it and all it means. If Colonel Crompton were satisfied to work to yards, feet, inches, and thousandths of an inch, by all means let him do so, but that was no reason either for or against the adoption of the metric system. He would not have us believe that, even among mechanical engineers, anything but a very small minority work to anything less than $\frac{1}{32}$ or at the outside $\frac{1}{64}$ of an inch, and have no use whatsoever for $\frac{1}{1000}$ of an inch. But how could he advocate the use of $\frac{1}{1000}$ inch when he started by putting all the blame of all this commotion down to the "abominable" system of decimals? Colonel Crompton wanted to know how the friends of decimals proposed to deal with recurring decimals. If his particular trouble was say, 0·3 inch, and he would give him 0·333 inch, he would be quite content to let Colonel Crompton "keep the change," and as long as his customer was satisfied what more could any one want? Anybody who thinks of $\frac{1}{1000}$ inch, by that very fact condemned the present system, for did he not thereby admit that in his opinion the unit of one inch is a thousand times too big for him? Mr. Wedekind said he had tried both systems, and found the metric system of enormous benefit. For all this he

had come that evening hoping to hear some good solid reasons in favour of our present system, but he went away unconverted, for never had he witnessed such "beating of the air." He had heard that decimals were an abomination, and thousandths of an inch, nay, ten-thousandths of an inch, excellent institutions, and he was amazed but unconvinced. What he did not hear was enough of the advantages which the use of the metric system bestow. It was not a question of a unit of lineal measurement, but the wonderful facility of expressing any quantity, whether of length, area, or mass, either in kind or value, and above all the charmingly simple relation of any one of these expressions to any other.

If only that were realised by that unfortunate minority whom the "shrieks" of the altruistic members of the majority have not yet reached, and who are blissfully ignorant of what they are depriving themselves of, the wilfully ignorant would very soon enjoy a distinction in which they might be left to revel to their hearts' content, that of being interesting specimens of rare humanity, to be pitied for their sore affiction of that worst kind of blindness which deprived them of the wish to see.

Writing of his own experience of both systems, Mr. Wedekind said that when he first went abroad and joined the engineering staff of a large machine tool works in Germany, the first thing he had to do was to throw away his 1 inch to 1 foot, 2 inches to 1 foot, 3 inches to one foot, and all the other scales, and replace them by a metre rule, price 2*d*. He was then fully equipped as far as rules and scales were concerned. All the beautiful tables of weights, with all their charm of printers', not to mention authors' errors, were replaced by the specific gravity of cast iron, wrought iron, steel and bronze, which he engraved in his mind and had never had to hunt for since, nor change for newer and better editions. That done, he was as much at home with the new system as the old, and a far better worker, for being suddenly relieved of a load he had been carrying about and the weight of which he had never realised until it was taken off his shoulders.

When he thought of some of those drawings which he got out, with their mass of dimensions, all in units with a quite occasional 0·5 mm., he positively shuddered to think of replacing a string of them by feet, inches, and fractions of inches, and more often than not a delightful little group such as 2 feet, $11\frac{23}{32}$ inches, and having to add them up and tally them with several other strings. The thing had to be seen to be realised in all its horror, when instead of 2, or 3, or 5, one had to find room on a drawing for such figures as $\frac{3}{16}$ inch, $\frac{1}{8}$ inch, and $\frac{3}{32}$ inch.

The weight of every piece of a machine had to be calculated,

but with a slide rule and the specific gravity of the respective metal, the most grotesque casting offered no trouble whatever.

From Germany he went to Paris, and was, of course, perfectly at home from the first moment. Later he went to Spain, and again, absolutely the same conditions obtained. On his return to London, when engaged on constructional steel-work designs for tall buildings, and ferro-concrete work, he did not go back to scales and tables. If he wanted to find the stress on any girder or stanchion, the bending moment of any load on an unequally loaded girder, the neutral axis of any re-inforced floor, or a centre of gravity, it was with a metre rule and dividers that he did it. Many rolling mills gave the safe loads for their joists, but whereas the mills' object was to sell their particular sections, his object was to design the lightest, i.e. cheapest, or, rather, most economical construction, and so instead of determining which factor of safety the maker of any particular section has used, or upon what tensile strength of steel he was basing his tables (which might differ from other makers and need not—to cut out some one else—necessitate his guaranteeing the tenacity of his steel to be say 32 tons per square inch in every charge merely because his tables were based on that figure), he simply calculated the safe load for any given section, and it did not matter whether it was a British or foreign section. There were no empirical formulæ required under such circumstances, but merely good sound fundamental scientific formulæ that you might stake your reputation on.

In railway work, what would the permanent way cost for any given gauge and weight of rail ?—Take the rails alone and compare: One mile of track of 45 lb. rails at £6 3*s.* 6*d.* per ton $= \frac{45 \times 2 \times 1760 \times 123 \cdot 5}{£2240 \times 20}$. By the metrical method, one kilometre of track would cost (for 45 kg. rails at 123·50 francs per 1000 kg.) 2 × 45 × 123·50 francs (or marks or lires) = 11,115 francs, because if one metre weighs 45 kilogrammes, one kilometre weighs 45 tons.

Other instances could be given where the metrical system had become absolutely indispensable to him, from calculating the percentage return on any given capital outlay for the manufacture of any given quantity of finished product based on the market price of the raw material, to the thickness of the spars required in a trestle bridge to carry an artillery train over a river, or the amount of guncotton to blow down a stockade, or blow up a regiment.

The President, in calling up Mr. Allen to reply, said that, at that late hour, it was impossible for the author to reply in full, but he would refer to one or two points, and give a complete reply in writing.

Reply.

Mr. Arthur H. Allen said that, when he said in his paper that British standards might be made to the nearest millimetre, he was thinking not of the Engineering Standards Committee, but of the standard parts of machines, the outside dimensions of bearings and so on—matters which were of no consequence whatever to a millimetre or two. He pointed out on the other hand that in the Engineering Standards Specification to which Colonel Crompton referred, the decimals were carried to ten-thousandths of an inch, showing that the use of the decimal measurements was already practised.

As to the difficulty of using decimals in connection with engineering manufacturing, which was quoted against the metric system, the best answer was to point to the countries where people were using the metric system most successfully in manufacture, such as Germany. Many English engineering firms had adopted the metric system wholly or partially, but the reason why English and American firms did not take it up more largely was because they thought that such a change ought to be simultaneous throughout the country. They might remember the familiar post-card: "When father says 'Turn,' we all turn"; they were waiting till father said 'Turn.'"

With regard to the matter which was said to be a libel on Armstrong, Whitworth and Company, namely the use of "bare" and "full" allowances, Colonel Crompton mentioned that thirty years ago Messrs. Armstrong had changed over to measurements in thousandths of an inch; but he (the author) had said in the paper that they had changed over forty years ago.

There was another point. It used to be said that parts of machines made to metric and inch measurements could not be made interchangeable. He (the author) had mentioned that they *could* be made interchangeable; and Colonel Crompton appeared to refer to that as an argument *against* the change to the metric system!

He was very much indebted to the Meeting for the kind way in which they had received the paper.

In a written communication, the author stated in reply to the President, that the present Chancellor of the Exchequer was, unfortunately, opposed to the compulsory adoption of the metric system; but this was evidently due, at least in part, to his being misinformed, for he had made statements regarding the adoption of the system in France which were not in accordance with facts that been established beyond dispute. He was much indebted to Professor Boys for his valuable evidence as to

the practical merits of the metric system, which corroborated the statements in the paper. With further reference to Colonel Crompton's remarks, the difficulty suggested with regard to repeating decimals, was purely academical; it was rarely necessary to use more than two decimal places (hundredths of a millimetre) in engineering measurements, and the maximum error due to taking the nearest figure in the second place could not exceed 0·005 mm., or 0·0002 inch. Moreover, precisely the same difficulty—if it existed—must necessarily be met with in using thousandths of an inch, as was done in almost every British or American engineering workshop with any pretensions to accuracy of workmanship. As for the supposed preference for the number 12, he was unable to detect it in practice. Apart from the division of the foot into 12 inches, and the practically obsolete troy pound into ounces, the number did not occur in any set of British measures with which he was acquainted. Twelfths of an inch were hardly ever used; the binary division was far more common, but it had in modern practice been superseded by the decimal division, as stated by Colonel Crompton, even where the English inch was still retained. Referring to the mensuration of land, the advocates of the metric system did not propose to alter areas, but only the units in which they were measured; new title-deeds could be drawn up in metric units as easily as in acres, poles, etc.—in fact, much more easily—and existing deeds need not be touched. The remarks of Major Hardy and Mr. Twigg were exceedingly interesting, and in harmony with the views expressed in the paper. As Mr. Mackerdy had pointed out, spring balances, etc., would require alteration, but this would in many cases apply only to the dials; the internal mechanism would be as applicable to the new units as to the old, and the range of measurement would, of course, remain unchanged. Regarding Mr. Silcock's extremely useful contribution to the discussion, which supported the author's claims on behalf of the convenience of the decimal system, he might point out that plans prepared on a scale of $\frac{1}{500}$ or $\frac{1}{2500}$ were independent of the system of units, and could be used as easily with metric units as with English. Mr. Ackermann's remarks went far to illustrate the absurdity of the existing British measures. Mr. Wedekind also, speaking from experience, amply bore out the author's contention that the metric system was immensely easier to use in practical calculations of all kinds. It was gratifying to observe that the tendency of the discussion was almost wholly favourable to the views which the author had been permitted to submit to the Society's criticism.

VACATION VISITS.

As usual, three Visits to Works were made during the summer vacation and descriptions of these follow.

The National Physical Laboratory.

By kind permission of Dr. R. T. Glazebrook, M.A., F.R.S., the Director, the members of the Society of Engineers paid a visit to the National Physical Laboratory, Teddington, on Saturday, June, 27, 1908.

They first visited the engineering department, which was housed in a large building of four bays, and the power station, various smaller rooms for special testing machines, photomicrographic work, etc., and offices.

Here were shown various machines for the testing of metals under repeated stresses, which are employed in researches carried out at the laboratory. They included: An alternating stress testing machine, giving 1200 reversals a minute; an impact testing machine for reversals of bending stress in a notched specimen; an impact testing machine for reversals of direct stress in a plain specimen; and a new fatigue testing machine. Among the apparatus on view may also be mentioned the following: A 1-ton testing machine with compound knife edges to produce tension or compression without moving the specimen; an abrasion testing machine; a 100-ton and a 10-ton Buckton testing machines; apparatus for testing pressure gauges, and for determining the specific heat of superheated steam.

The standard screw lathe, for correcting leading screws of lathes, was also shown. This was in a separate building.

In the electrotechnical building were seen various apparatus and machines for alternating current instrument tests, and the equipment for testing ammeters, voltmeters and supply meters, water-tube regulating resistances for high current work, arrangements for testing resistance boxes, insulation resistances and low resistances, etc. The photometry department was housed in this building, and the apparatus for the measurement of the candle-power of various sources of light, with the arrangements for life-tests of electric glow lamps were shown.

Bushy House, the main building of the laboratory, was originally the official residence of the Ranger of Bushy Park. After the death of King William IV., Queen Adelaide lived there until 1849, when it passed to the Duc de Nemours.

On entering Bushy House, the party divided, and the following departments and apparatus were shown:—

In the Metallurgy Department.—Various melting and heating furnaces, with apparatus for the measurement of high temperatures, and for obtaining cooling curves. Apparatus for grinding metals *in vacuo.* Apparatus for the microscopic study of metals.

In the Metrology Department.—Comparators for the accurate comparison of line standards of length, and the calibration of scales. Tape measuring apparatus. Hartmann automatic and other comparators for end gauges. Apparatus for measuring screws. Various scales, gauges, and templates.

Tide-predicting Machine, used for the preparation of the Indian tide tables.

Electricity Department.—Ampère Balance.

The visit terminated about 5 o'clock, and the party then adjourned for tea to the lawn of the Anglers' Hotel, Teddington Lock.

The Whitefriars Glass Works.

By kind permission of Messrs. James Powell and Co., the members of the Society of Engineers paid a visit to the Whitefriars Glass Works, Tudor Street, E.C., on Tuesday, July 14, 1908.

In the Mixing Room the raw materials of glass were found, showing among other things the formation of lead-glass and soda-lime glass.

The Pot Room was visited, and in it was seen a stock of finished crucibles, as well as some in course of being built up rod by rod. The crucibles were in the shape of a beehive, with a semicircular opening near the top, and on entering the Glass House these semicircular openings were seen peering out from the circumference of the furnace, and served for the introduction of raw materials and other purposes. The goods, when finished by the workman, are slowly cooled or "annealed," either by being gradually moved away in iron pans from a fixed source of heat in the "bars," or by being placed in a chamber or "kiln," in which the source of heat is allowed gradually to die out. The Weighing Room was next visited, where every imperfect piece is broken and put aside for remelting, while every perfect piece is passed on to the Cutting Shop. In this shop it was

extremely interesting to watch the process of decorative cutting by means of iron or stone wheels, fed respectively with sand mixed with water, and with water alone.

The Sheet-Glass Cutting Shop was also visited, where the glass is cut either with a diamond or a small steel wheel.

Other departments were visited, where demonstrations of the various work were given, and in which the members were greatly interested, the visit terminating about 5.30 p.m.

The Admiralty Harbour, Dover.

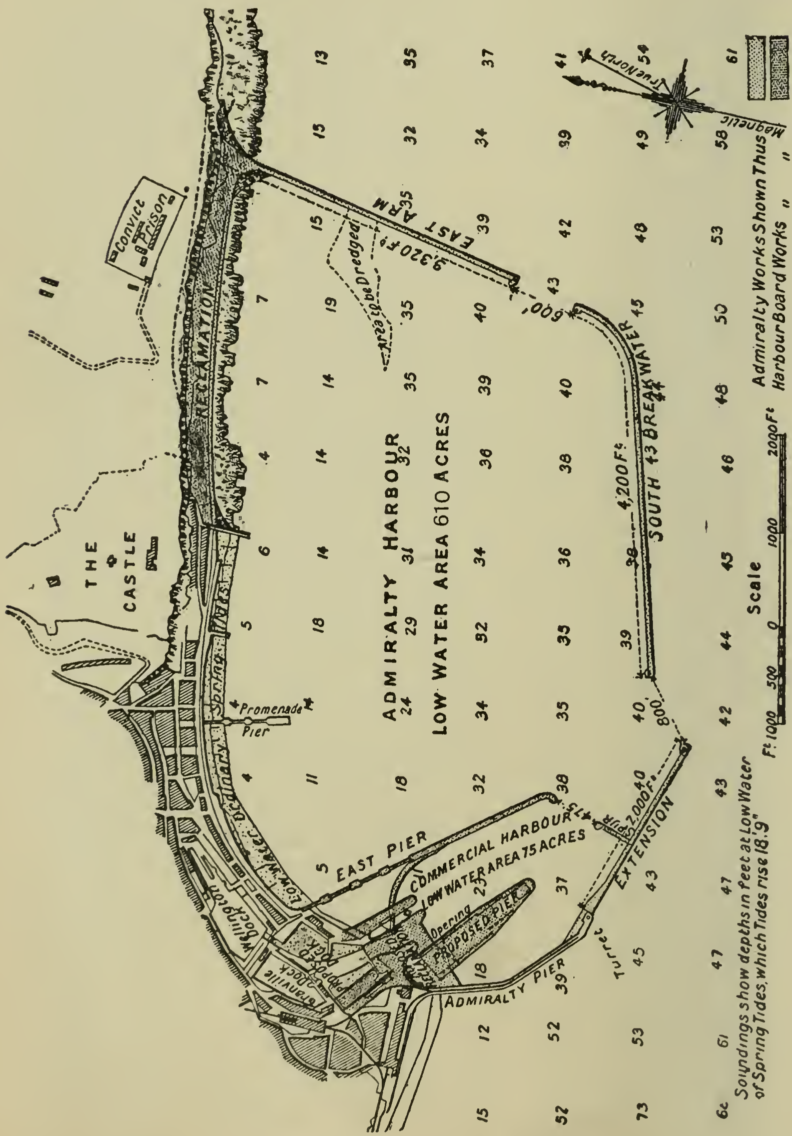
A visit was made by the President—Mr. J. W. Wilson—and a number of members of the Society of Engineers, on Tuesday, September 22, 1908, to the works of the New Admiralty Harbour at Dover, which were inspected by the permission of the Admiralty, and the visitors were under the guidance of Messrs. Coode, Son and Matthews, the engineers, and Messrs. S. Pearson and Son, Ltd., the contractors.

The historical portion of the following description of the works, and the plan, have appeared in the *Engineer*.

The nucleus of the present work is the Old Admiralty Pier. Practically the history of the harbour dates from the year 1840, when a Royal Commission was appointed to survey the harbours on the South-East Coast. This Commission recommended the plan of a harbour at Dover formed by a breakwater running practically parallel with the land, and at an average distance of 1000 yards from the shore, and by two arms projected from the land towards the eastern and western extremities of this breakwater. The estimated cost of the proposed works was 2,000,000*l.* Almost immediately after this Commission had reported—namely, in the year 1844—another Commission was appointed to report as to whether it was desirable that a harbour of refuge should be constructed in the Channel, and what site would be most eligible, having regard to accessibility, to its use, if necessary, for armed vessels, and to the facilities for defending itself which it possessed. This Commission concurred in pronouncing a favourable opinion on Dover as a harbour of refuge. They were, however, uncertain as to the suitability of the ground for anchorage purposes, and raised the question as to the possibility of the harbour silting. The first point was put at rest by the emphatic testimony as to the excellence of the ground, from an anchorage point of view, of Captain Washington in command of H.M.S. *Blazer*.

With regard to the question of silt, a further Commission appointed in 1845 to consider plans submitted by eight of the

leading engineers of the day as to the form and construction of the harbour, reported that "if liability to silt were deemed an



DOVER HARBOUR.

objection it would be idle to attempt such works on any part of our coast." This Commission practically confirmed the report

of the 1844 Commission as to the form of the proposed harbour. As a result, the contract for the Admiralty Pier was let in 1847. The structure was, save for a small addition to its seaward end, completed in 1871. Its total length is 2000 feet. Nothing further was done towards completing the harbour, however, in spite of the fact that in 1881 a Committee appointed to report on the employment of convicts suggested the building of a prison on the East Cliff and the employment of convict labour to build a pier and breakwater at Dover so as to form with the Admiralty Pier a large harbour similar to that at Portland.

The Admiralty Pier afforded very fair protection against a south-westerly gale, but with a south-easterly gale the pier was exposed on both faces, with the result that serious delay and inconvenience was felt while these winds were blowing. Seeing that nothing was done by the Admiralty, the Dover Harbour Board decided in 1890 to construct a small harbour of their own. which work was sanctioned by Parliament in the following year. The work, which has been slightly modified by the later decision of the Admiralty to build the larger harbour, is shown in the plan, and is marked Commercial Harbour. The proposed pier, reclamation and lock have not, as yet, been constructed, nor is the whole of the other work yet completed.

Towards the end of 1895 the Admiralty instructed Messrs. Coode, Son and Matthews to prepare plans for a large enclosed harbour. As a result the following recommendations were made: (1) That the Admiralty Pier should be extended 2000 feet in an east-south-easterly direction, practically doubling its length. (2) That an east arm, commencing a few hundred feet to the east of the eastern boundary wall of the convict prison, should be run in a direction approximately south by west for a distance of 3320 feet. (3) That an isolated breakwater 4200 feet in length should run generally west by south and east by north, but turning towards the north at its eastern end, and should form the southern protecting arm. These recommendations are all shown on the accompanying plan. The average depth at low water of ordinary spring tides on the line of the southern breakwater is 42 feet. In addition it was recommended to reclaim 21 acres of the foreshore. The suggested arrangement of breakwaters left an entrance of 600 feet in width between the east arm and the south breakwater, and an entrance 800 feet wide between the extended head of the Admiralty Pier and the south breakwater. Both entrances have a depth of about seven fathoms at low water of spring tides, which rise 18 feet 9 inches.

These proposals were accepted, and the contract for the construction of the harbour was let in 1897 to Messrs. S. Pearson

and Son. The total length of the sheltering works to be constructed is 9520 feet, and the area enclosed, exclusive of the Commercial Harbour belonging to the Dover Harbour Board, will be 610 acres at low water, 322 acres being outside the five-fathom line, and 171 acres outside the six-fathom line. The project put forward by the 1840 Commission embraced a harbour containing a total of 450 acres, of which 320 acres would have been seaward of the two-fathom line. We may here mention that the area of the Commercial Harbour, which was originally to be 56 acres, will, when the works are completed as altered, be 75 acres. It will be seen that the end of the Admiralty Pier extension is some 400 feet to 500 feet south of the western end of the south breakwater. This arrangement has been made with the object of helping vessels entering the harbour when the tide is setting eastwards at its greatest velocity. At such times the speed of the current is nearly four knots. It will also be useful during south-westerly gales.

The works were to be completed in ten years, or at the end of 1907, and although at one time there was every prospect of this being done, from unforeseen causes which arose as the work progressed, damage done by shipping, and various additional works, it will still be some months before they are finally completed.

The Admiralty Pier Extension and East Arm are completed and during next summer the South Breakwater will be so nearly finished that the viaduct spanning the east entrance will be removed, the larger part of the reclamation will be cleared and utilised by the Admiralty as a naval yard, provision being made for a submarine flotilla, so that for all practical purposes the harbour may be ready for service next summer.

In its present state it is difficult to realise the magnitude of the temporary works which were necessary to put in place the 1,500,000 cubic yards of concrete and the 3,000,000 cubic feet of granite. To do this $4\frac{3}{4}$ miles of double line railway track has been built on staging in the open sea, subject to whatever the weather offered, the plant to handle this material having reached at one time 2500 tons. The losses due to the action of the sea, both to permanent and temporary work, have been very small. A worse enemy to progress, and one which is always present, has been the tidal currents, as they so limit the time in which divers can work, and, of course, the tidal rise and fall interfere with all work between high and low water. Under these conditions a short visit might be paid to the works, even when in full progress, without seeing any work being carried on, or but little of any great interest.

In addition to what can be seen of the breakwaters above

water is an apron all round the outer toe, consisting of concrete blocks of about 12 tons each, covering a width of 25 feet for protection against undermining.

The Harbour Board are about to have constructed a widening of the shore end of the Admiralty Pier, on which a large station is to be built for dealing with the cross channel traffic, and also a timber staging 800 feet long and 20 feet wide alongside the Admiralty Pier Extension for the use of the Atlantic liners, with a passenger shelter and platform adjoining.

With these additions, and the Naval Harbour in use, Dover should become an important shipping port.

The works are being executed under the Direction of Major Sir Henry Pilkington, K.C.B., R.E., the civil engineer-in-chief appointed under the Naval Works Loan; and of Messrs. Coode, Son and Matthews, the chief engineers by whom the design was prepared. The estimated cost of the whole work is about 3,500,000*l*.

October 5, 1908.

JOSEPH WILLIAM WILSON, President,
in the Chair.

THE HISTORY OF MECHANICAL TRACTION ON TRAMWAYS AND ROADS.

By H. Conradi, A.S.E.

The first steam carriage for ordinary roads was built in 1769, by a French inventor, M. Cugnot, who was followed in later years by several others, of whom, however, Trevethick, during 1803 and following years, was the most distinguished and most successful inventor and engineer. Then followed, as important designers and manufacturers of steam omnibuses for common roads, Gurney, in 1828, and Walter Hancock, from 1829 to 1836. All these steam omnibuses were cleverly constructed, and practically carried out, transporting thousands of passengers over thousands of miles. Their services were only discontinued on account of heavy taxes, imposed by the local authorities. A similar case in reference to private, not municipal, tramway companies exists even now, as their free development has always been hampered by the short-sighted Acts of Parliament authorising the local authorities to become oppressive competitors as municipal traders, and to buy them up after the comparatively short period of 21 years' lease at the price of old rails and worn-out rolling stock. Local authorities should be only the trustees of the public purse, but not active competitive traders preventing private enterprise. Time went on till 1860, when Mr. G. T. Train patented another type of steam car. In 1871, Mr. Nairn designed and manufactured a 3-wheeled steam omnibus, weighing, fully loaded with 50 passengers, about 10 tons. The engines consisted of 3 cylinders of $5\frac{1}{4}$ inches diameter by 10 inches stroke. The boiler was of the Field boiler construc-

tion of best Bowling iron, 27 inches in diameter, 6 feet 3 inches high of $\frac{5}{16}$ inch thick plates. The water-tank contained 150 gallons, and the boiler was fed by an injector. The wheels were 38 inches in diameter by 10 inches wide. This steam omnibus did the service between Edinburgh and Portobello, about 3 miles, 12 times per day.

During 1871–72, the Grantham Steam Tramway Car was patented and constructed, being the first built in England. The car body was manufactured at the Oldbury Carriage Works, and the engines and boilers by Messrs. Merryweather and Sons.

The principal dimensions were :—

Diameter of steam cylinders	4 inches.
Stroke of piston	10 inches.
Diameter of boilers	18 inches.
Height of boilers	4 feet 4 inches.
Diameter of wheels	2 feet 6 inches.
Length of car	30 feet.
Capacity of car	20 passengers inside and 24 outside.
Weight of car, empty	$6\frac{1}{2}$ tons.
Weight of car in full working order ..	8 tons.

The working of this steam car on the Wantage tramways came to about 4*d.* per car mile. It is illustrated in Figs. 1–4, showing side elevation, transverse section, and plan.

During the years 1871–72 and subsequently, great activity seems to have been displayed in the construction of steam cars for tramways and ordinary roads.

Figs. 5–13 illustrate a steam omnibus for common roads, brought out at that time by Mr. Leonard J. Todd, of Leith. Two important features in this steam car, were the use of coupling-rods, to drive the road-wheels direct from the crank-shaft, and the adaptation of a steam fan to obviate the noise of the exhaust steam, and to prevent steam and smoke. The principal dimensions were :—

Diameter of cylinders	10 inches.
Stroke of piston	10 inches.
Boiler surface	300 square feet of heating surface.
Grate surface	9 square feet.
Working pressure of steam	180 lb. per square inch.
Capacity of 1 pair of wing-tanks	300 gallons.
Weight in full working order on the road	14 tons.
Number of revolutions of crank-shaft ..	186.

The steam tram-locomotive, built by Mr. Todd for the Santander Tramway Company in Spain, was designed to draw 2 passenger cars with 76 passengers in the two.

Its principal dimensions were as follows:—

Diameter of cylinders	$6\frac{1}{2}$ inches.
Stroke of piston	9 inches.
Boiler surface	160 square feet of heating surface.
Grate surface	$3\frac{1}{2}$ square feet.
Diameter of driving wheels	5 feet 6 inches.
Crank-shaft revolutions..	150 at a speed of 10 miles per hour.
Boiler pressure..	150 lb. per square inch.
Weight in full working order	5 tons.

The motion of the crank-shaft was transferred to the driving wheels by a pair of spur-wheels.

In 1875, Mr. Todd constructed also a fireless engine.

A new kind of steam tramway locomotive was built in 1874 by the Yorkshire Engine Company for the Belgian Street Railways, on Mr. Loftus Perkins' principle. Its special construction consisted in being provided with a large air-surface condenser of 800 square feet, composed of two rows of copper pipes, one on each side of the engine, into which the exhaust steam was conducted and condensed.

Mr. Charles C. Cramp constructed about 1874 a steam-tram locomotive working with superheated steam and with rotary engine cylinders. To reduce the tractional resistance, and to work economically, he used as necessary accessories special rail cleaners to obtain and to keep a clean rail-surface and groove; a most necessary and practical arrangement for a good and economical service.

Figs. 14 to 16 represent the original design of the steam tramcar constructed in 1876 by M. A. Brunner, of Berne, Switzerland, on the same principle as the double-bogie railway steam carriage built for Mr. R. T. Fairlie in 1869. The car is provided at each end with a four-wheeled swivelling bogie, each of which can, if necessary, be provided with steam cylinders.

The Belpaire Steam Tramcar was constructed in 1876 at the Malines Arsenal by order of M. Belpaire, Inspector-General of the Belgian State Railways, under the superintendence of M. Shaar, Director of Works. The boiler is a horizontal tubular one placed transversely on the carriage underframe.

Figs. 17 to 19 represent Brunner's original steam tramcar, but with the modification that a complete small locomotive is placed at one end of the car, while the other end is supported by a small 4-wheeled bogie. The engine can be easily detached from the frame. This steam-car worked on the 1-metre (3 feet $3\frac{3}{8}$ inches) gauge local traffic branch line of the Lausanne and Eschellens Railway. The motion of the pistons is transmitted to the driving wheels by means of equal armed vertical

levers, such as were introduced by MM. Carels, of Ghent, in an engine shown at the late Vienna exhibition, and constructed for the Belgian state railways from M. Belpaire's designs. The valve motion is of a type lately patented by Mr. C. Brown, the manager of the Winterthur Locomotive Works, where this car was constructed. Accordingly, the steam distribution is effected with neither eccentrics nor expansion links, but by means of a system of levers actuated by connecting-rod and reversing handle. The boiler is of the ordinary locomotive pattern. The water-tanks are built between the frame-plates, the coal-bunkers are on each side of the fire-box. There is a pump on one side worked from the motion beam, and an injector on the other side. For comparison the principal dimensions are given :—

Diameter of cylinder	$6\frac{1}{4}$ inches
Length of stroke	12 inches
Diameter of coupled wheels	2 feet $3\frac{1}{2}$ inches
Wheel base	4 feet 1 inch
Inside diameter of boiler	2 feet $3\frac{1}{2}$ inches
Length between tube plates	4 feet 7 inches
Total heating surface (fire-box 20 feet square)	150 square feet
Grate surface	2·35 square feet
Capacity of water tanks	130 gallons
Capacity of coal bunkers	5 cwt.
Weight of engine, empty	5 tons
Weight of engine in working order	6 tons
Working steam pressure	180 lb.
Gauge of rails (1 metre)	3 feet $3\frac{3}{8}$ inches

Considering the tractive power and using the ordinary general formulæ for locomotives: T = tractive power ; D = diameter of cylinders in inches ; L = length of piston-stroke in inches ; W = diameter of driving-wheels ; P = effective mean pressure of steam in the cylinders in pounds per square inch. Then:

$$T = \frac{D^2 P . L}{W}$$

which in this case produces

$$T = \frac{(6 \cdot 25)^2 \times 12}{27 \cdot 5} = 17 \text{ lb.}$$

and with the steam at 180 lb. in the boiler, the mean effective cylinder pressure taken at 60 per cent. of the boiler pressure, would be about 110 lb. maintained at the pistons, making thus the

Total tractive power = 17 lb. × 110 lb. = 1870 lb.

In reference to the construction of the car, the main frame

is built of double T iron, and the carriage floor is 19 inches above rail level. The carriage is composed of one compartment with longitudinal seats for 24 inside passengers, with one pavilion over the trailing bogie as a smoking compartment for 7 inside passengers, and on the top of the carriage for 30 outside passengers. A staircase from the pavilion leads to the outside seats. The leading dimensions of the car are as follows:—

Length over all	42 feet $3\frac{1}{2}$ inches
Breadth (maximum)	7 feet $9\frac{1}{2}$ inches
Height	14 feet 1 inch
Diameter of bogie wheels	1 foot $5\frac{3}{4}$ inches
Wheel base of bogie	4 feet
Distance between bogie pins	30 feet $4\frac{3}{4}$ inches
Weight of car empty, including engine	$11\frac{1}{2}$ tons
Dead weight per seat	430 lb.
Total weight with 60 passengers and luggage ..	16 tons
Adhesive weight at full load	10 tons

The coal consumption during a journey from Lausanne to Eschellens and back, 18 miles, with a normal load of 60 passengers, was 160 lb., equal to 9 lb. per mile run. The line has a continuous gradient of about 1 mile in length, varying from 1 in 25 to 1 in 40; the maximum gradient is 650 yards long. On mounting a gradient of 1 in 25, the corresponding resistances may be computed as follows:—

$$\text{The resistance due to gravity} = \frac{2240 \text{ lb.}}{25 \text{ gradient}} = 90 \text{ lb. per ton.}$$

Taking the frictional resistances at about 20 lb. per ton it would give:—

Gravity of 16 tons at 90 lb. per ton .	= 1440 lb.
Frictional and other resistances at 20 lb. per ton for 16 tons . . .	= 320 lb.
Total . .	= 1760 lb.

As the tractive power for each pound of effective pressure per square inch of piston has been calculated by the above formulæ to be 17 lb., the mean effective pressure required, per square inch of piston, will thus be

$$\frac{1760}{17} = 103 \text{ lb.}$$

With a full load the steam car takes a gradient of 1 in 25 at a speed of 8 miles per hour. To overcome the resistances, as

calculated above at 1760 lb., the engine, at 8 miles per hour or 704 feet per minute, exerts an effective power of:

$$\frac{1760 \text{ lb.} \times 704 \text{ ft.}}{33000 \text{ lb.}} = 37 \cdot 5 \text{ H.P.}$$

The adhesive weight resting on the coupled wheels is about 10 tons, or 22,400 lb.; dividing the total resistances by the adhesive weight, we get:

$$\frac{1760}{22400} = \frac{1}{12 \cdot 6}$$

as the coefficient of adhesion required. The mean working speed of the trains on the line from Lausanne to Eschellens is on the average 10 miles per hour according to level and gradient.

The cost of working the steam-car at Lausanne has been 19*s.* per day, comprising the men's wages, coal and grease, as follows:

	s.	*d.*	
Engine driver	6	5	Assuming the engine to run 60 to 70 miles per day the cost per mile would be on an average 3½*d.* per mile.
Stoker	2	10	
Conductor	3	2	

The car is not turned round at the terminus, but runs in either direction. The steam-car elegantly fitted up costs 1280*l.*, but less luxuriously would cost about 1000*l.*

A Danish engineer of Copenhagen, Mr. R. W. Rowan, constructed about the same year a steam tramway car on the combined principle of the Fairlie system as regards the boiler, and on the Brown principle so far as the general arrangement of the engine and its working parts are concerned.

Fig. 20 represents the English and French types of Mr. C. Brown's steam tramway locomotives, built by the locomotive works of Winterthur, Switzerland.

These at the time were some of the best built engines, as regards detail of construction, type, material and cost price. All the gearings were well protected against mud and dust. The link-motions were of different types. The engines were of the most substantial construction.

Fig. 21 illustrates one of the types of tramway engines constructed by Mr. Henry Hughes, of Loughborough, for the English and French tram lines. Those for the English lines were in service in 1876 on the Edinburgh, Sheffield, Leicester and Wantage tramways. In 1877 a regular service began for a number of years on the Vale of Clyde Tramways. This locomotive was more particularly constructed on the principle of tank engines

for collieries, with inside cylinders and 4-coupled wheels. The principal dimensions were as follows:

Diameter of cylinder	7 inches.
Stroke of piston	12 inches.
Diameter of boiler of Lowmoor iron ..	2 feet $3\frac{3}{4}$ inches.
Firebox, of copper	2 feet 5 inches high × 1 foot $10\frac{3}{4}$ inches long × 2 feet 1 inch wide.
Ditto plates, thickness of	$\frac{3}{8}$ inch to $\frac{5}{8}$ inch at tubes.
Number of brass tubes	62.
Diameter of ditto	$1\frac{1}{2}$ inch.
Length of ditto between tube plates ..	5 feet $7\frac{3}{8}$ inches.
Grate area	3·70 square feet.
Heating surface	$149\frac{1}{2}$ square feet.
Working pressure	80 to 100 lb. per square inch.
Diameter of wheels	2 feet 6 inches.
Capacity of water tank (saddle tank) ..	400 gallons.

This engine had a special and ingenious condensing arrangement, patented in 1876. It is composed of a casting fitted outside of the bottom delivery tank in front of the engine, forming a condenser. It was provided with two valves, one above the other, but both connected together, and resting in their respective seats. The action is as follows. As soon as the exhaust steam passed from the cylinder through the exhaust pipe leading into this condenser the exhaust steam lifted both valves. It thus opened the connection between the cold water saddle tank, on top of boiler, and the condenser, into which latter the exhaust steam escaped and was condensed by a jet of cold water coming through the water pipe, which was connected at top with the cold water saddle tank, and at the bottom to the condenser. As soon as the steam was condensed, a vacuum was formed in the condenser, and the external pressure closed both valves again. The condensed steam and injection water passed into the lower front tank at a temperature of 150° to 170° F. through a piston valve fitted into a corresponding casting, which was fitted inside this lower front tank. The piston valve opened and closed the communication between this front tank and the outside condenser, and was regulated by a rod connected with an arm of the steam regulator handle; so whenever the regulator was opened, it opened this piston valve in the tank at the same time. A similar low-level tank was fitted to the engine at the fire-box end, and both these tanks were connected by a pipe. The boiler feed-pump, or injector, drew its supply from these tanks. At the end of the journey these tanks were emptied by means of a large delivery pipe fitted to the tank at the fire-box end of the engine. The quantity of water consumed in conveying two cars with 80 passengers was about 25 gallons per mile at a temperature of 150° to 170° F.,

and the coke consumed was about 8 lb. per mile. The two photos exhibited show the Hughes' steam tram engines at work in Paris on the Bastille-Charenton line of the Southern Tramway Company of Paris, about the middle of 1879. The cost of the Paris tram engines was 650*l.* to 700*l.* The very great difficulties the service had to face were, firstly, the extreme weakness of the permanent way, which was laid for horse tramcars only, and was in a wretched condition. The light rails of a hollow section of 40 lb. to 45 lb. per yard, constructed to bear the weight of a fully loaded horse tramcar of 2 tons, could naturally not stand the weight of an engine of 7½ to 8 tons. The destructive wave-motion of the rails in advance of the engine was beyond description, and considerably increased the tractional resistance. The price paid for the regular traction work was 7·25*d.* per mile for drawing one car, and 9·25*d.* for two cars. On holidays the engine drew 3 cars, so that a satisfactory and profitable service could have been done with a strong and good permanent way. Besides the trouble caused by the wretchedness of the permanent way, the boilers also started to give way. This was caused by the feed-water taken from the River Marne, being very heavily charged with lime. Further, the feeding with the condensed exhaust steam caused grease from the cylinders to get mixed with the feed water, thus allowing a deleterious compound of lime and grease to be formed. This greasy compound settled on the sides of the tube plates and on the top and sides of the fire-box. The plates and the fire-box consequently became over-heated, through not being wetted, and bulged inwards, thus causing the tubes to leak and to break. The author considered it a great neglect on the part of the French tramway company not to have drawn the attention of the English contractor to the fact that the feed water of the river was so calcareous, thus adding fresh and unknown causes to the destruction of engines and boilers. The author having been instructed to investigate the then unknown causes, directed his attention to the condition of the fuel and the feed water supply. In consultation with the Paris Gas and Water Company and the engineers of the River Seine Steamship Company, which was also working with condensing steam-engines, the disastrous causes of the lime water and the compound formed with the grease, of which this company had also had bitter experience, were thus traced by the writer. The first discovery of the destructive effect of such greasy deposits and of the dangerous consequences for the boilers, of lime water were made by Mr. Weber, engineer, of Vienna. He conferred on the subject with the great locomotive and engineering firm of Borsig, in Berlin, where experiments on such deposits in large boilers were made,

and the cause and its effects confirmed. Soon after similar experiments were carried out by the great engine builders of St. Denis, near Paris, Messrs. Farcot and Son, with the same results. They operated consecutively during six months on two large boilers. Consequently in working the tram-engine boilers chemical and physical methods were applied to prevent any deposit, after they had been chemically cleaned and washed out and more firmly stayed. The compositions used on the recommendations of the French steamship engineers, who used them for their stationary condensing engines with very good effect, reduced the deposits in the locomotive boiler, but not quite sufficiently; while the same in their soft soapy solutions, if not in a sufficiently liquid state, often passed with the steam in a rather pasty state into the cylinders, preventing the free motion of the pistons. To get completely relieved of all these troubles, the boilers were then fed from a fresh-water tank. The French Tramway Company, desirous to continue the steam service in spite of the difficulties experienced at the beginning, was, through the author's efforts, persuaded to accord to the Hughes' Engine Company further concessions of their tram lines for steam service. To prepare the Paris lines for engine service, the two companies tried to make a financial arrangement, but this fell through. The French company took the Paris service over in final settlement. A new contract for the Lille tramways on the Lille-Haubourdin and the Lille-Roubaix line was then entered into.

On account of some gradients, more powerful engines were used on these lines. The condensing arrangement for these engines consisted in passing the exhaust steam through a feed-water heater, composed of a number of brass tubes enclosed in a cast-iron tank fixed to the top of the saddle water-tank. From there it passed into the saddle tank, from which the boiler was fed. The exhaust steam was either discharged into the saddle tank upon the water surface, or it was carried through branch pipes below the surface of the water, to the bottom of the tank. The discharge took place upon the surface of the water in the country, and under the water surface in town, in order to prevent any escape of exhaust steam. The principal dimensions of these larger engines (Fig. 21) were as follows :—

Diameter of cylinders	9 inhces.
Stroke of piston	13 inches.
Diameter of boiler barrel	2 feet 4½ inches.
Diameter of brass flue-tubes	1½ inch.
Number of ditto	52.
Length of ditto	5 feet 8 inches.
Length of copper fire-box	2 feet 1¼ inch.
Width of ditto	1 foot 11¼ inches.

Height of same above grate	2 feet 7 inches.
Grate area	3·82 square feet.
Heating surface	162½ square feet.
Boiler pressure	80 to 100 lb. per square inch.
Diameter of wheels	3 feet 6 inches.
Number of coupled wheels	4.
Capacity of tank	400 gallons.
Weight of engine, empty	7½ tons.
Weight of engine in full working order	10 tons.
Diameter of steam brake cylinder	5 inches.
Cost price of locomotives	750*l.* to 800*l.* and over.

After satisfactory working of these lines, the French Tramway Company took the service over.

The Steam Tram Engine, designed and constructed by Messrs. Schwartzkopf, of Berlin, was a condensing-engine with coupled axles. The boiler was tubular, with circular fire-box. The cylinders were placed on both sides of the smoke-box. The transfer of the motion was on the Belpaire principle. The slide valve-gear was arranged with the application of eccentrics and cranks. The distinguishing feature was the method of condensation, as will be seen from the cross-section, the condenser being formed by the tubes inserted in the framework of the locomotive. This system of pipes formed the side frames for the top part of the engine, and the water circulated in these pipes by being pumped in, and through them the exhaust steam passed before entering the condenser.

Another engine of special design was the Wilkinson Condensing Tramway Locomotive. The distinguishing features were the application of vertical engine cylinders of inverted type, similar to small marine engines, the crank-shaft being placed at right angles to the driving axle, instead of being, as usual, parallel to it; further, the application of a worm-and-wheel gearing as driving machinery of the driving axle, instead of driving directly by means of piston rod, crossheads, and connecting rods fixed to the cranks of the driving axle. The boiler was a combination of the locomotive and the Field water tubes, with a very short boiler barrel, and the ordinary horizontal tubes, but bent into a reverse S curve.

While the writer was superintending the steam service of the Hughes' Tramway Locomotives in Paris and Lille in 1880, two other very interesting and important tramway traction services were at work in Paris and its suburbs. The one was M. Léon Francq's Fireless Locomotive, illustrated in Fig. 22, with the installation of the workshop, the boiler-house for the large stationary boilers, to produce the required quantity of high-pressure steam for the fireless engines, of about 135 lb. per square inch effective pressure. The engines on

which the writer travelled worked on the Ruel-Marly line, some miles from Paris. The service was well conducted; the difficulty with these engines, based on the principle of spontaneous evaporation, consisted specially in the great quantity of water they had to carry for the production of the required amount of steam.

The other type of engines were Merkasky's Compressed Air Engines. The principle of the construction and working of these engines consists in previously introducing compressed air into the engine, which is gradually delivered into its working cylinders. The power thus obtained is transmitted to the driving-wheels by the usual steam locomotive mechanism. The difficulties in working with compressed air are that the high temperature of the air, due to compression, requires to be lowered before the air can be used, while care must be taken that the temperature of the air, in working expansively, does not drop too low. To prevent these difficulties, the compressed air was warmed during its admission into the cylinders by the concurrent injection of steam. The writer drove with this car on the Paris-Belleville line, and the service was naturally free from smoke and exhaust steam; it worked quietly.

It remains now to mention as the last series of steam work, tramway service by cable-traction. The principle consists in the application of an endless wire rope, laid over two comparatively large V-grooved driving pulleys, working continuously in the same direction, being assisted by a number of smaller guide and tension pulleys to help support the long cable, and further of a number of deflecting pulleys to transfer and to support the cable in the direction the tramcar has to travel, and to assist in its passage over the curves and cross-over roads. The pulleys, as bearers of the cable, and the cable, after having left the great driving pulleys in the engine-house, are then placed with their requisite accessories below the road in a slotted passage, conduit, or tube, built of masonry or concrete. The rope is driven by means of a stationary steam-engine at the depot, or other motive power; while the motion of the cable is transferred to the cars, by means of a mechanical apparatus fixed to the car, called "the gripper," which grips and holds the cable or releases the same, according to the requirements of the service, to move or to stop the car. The writer visited the important installations and the service at Edinburgh, Birmingham, and here in London at Brixton and Highgate Hill. In enumerating the traction methods formerly used, the application of the Gas-engine for tramway traction should be mentioned, as it has been used on the Croydon line, where the writer had the opportunity to observe it in the summer of 1893. The motor

was driven by compressed coal gas. The motor, with 3 cylindrical gas holders and all the working parts of the machinery, were placed underneath the car-frame. The gas supplied to the reservoirs was sufficient to drive the car 8 miles. The motor had 2 cylinders at opposite sides of the crank-shaft and both driving on the one crank-shaft. The ignition of the charge in the engine-cylinder was electrical, and the products of combustion were carried into the condenser. An 8 H.P. Otto gas-engine installed at the Croydon depot drove a compressor, which pumped the gas taken from the gas company's main into a steel cylindrical reservoir 4 feet in diameter × 25 feet in length, at a pressure of 150 lb. per square inch; a charge which was sufficient for 5 tram-car gas motors. It was stated that the gas cost 1*d*. per mile.

Mechanical traction having been started, say at about 1800, with steam omnibuses on ordinary roads, it may be opportune to mention comparatively the two most important types of steam omnibuses of our present time, say of about 1900, just a century after the first period. The two principal systems of steam motors introduced and in service are the French type of the distinguished French engineer, the late M. Serpollet, of Paris, and the English type, which seems a great improvement on the former, of the distinguished English engineer, Mr. T. Clarkson, of Maulsham Works, Chelmsford. The chief feature of the actual steam motor omnibus consists in the application of the so-called flash-boiler, producing instantaneous evaporation of the feed water. This instantaneous generator was the invention of the late M. Serpollet, of Paris, and has done away with all the difficulties (caused by impurity of water and otherwise), with the boilers of the steam-tram locomotives.

The general arrangement of the motor-bus is as follows:—The engine is usually mounted at the front end and the cylinders are mostly in a vertical position. The crankshaft is longitudinal, in the centre line of the car, and is coupled through a clutch to the propeller shaft, which extends to the rectangularly or crosswise-placed back axle, through bevel or worm gearing, called the live-axle drive. Another method is, that the right-angle change of the direction is made from the crankshaft to a countershaft, parallel with the back axle, which is fixed, the latter being driven from the countershaft by a chain to each driving-wheel. The car is steered by a steering-wheel and lever arrangement. In petrol-driven motor-buses a separate shaft with gearing-wheels is put between the crankshaft and the propeller-shaft for the different speeds, which, however, is not the case with steam-driven motor omnibuses. The differential gear, necessary for road vehicles, is, in the case of live-axle driving, fixed to the live axle, in the case of countershafts

it is fixed to the countershaft. The engine generally has 4 cylinders, sometimes even 6 to 8 are used. The explosive mixture, composed of petroleum spirit and air, is led to the engine from a mixing apparatus, called the carburettor, in which the petroleum spirit is vaporised and mixed with the air in given proportions. The mixture effected is then admitted to the cylinders by a corresponding number of valves, while ignition devices are used, mostly of an electrical type, to cause the explosion and thus produce the power. To keep the cylinder heads, containing the valves, at a proper working temperature, water circulation is provided round the cylinder jackets, the water being cooled by being forced through an air radiator fixed in front of the car. The power from the crankshaft is transmitted through a clutch, either direct to the propeller-shaft or through an intermediate shaft, when the gear-ratio has to be changed or the engine to be reversed. This change in the gear-ratio is made by moving into mesh various gear-wheels which are fitted to slide on the third shaft. In the case of a live-axle drive, the propeller-shaft drives the back axle, through bevel-gearing fixed in the same case with the differential gear. When fixed back axles are used with chains, the longitudinal drive of the crankshaft is transmitted through bevel-gear to the countershaft, which is parallel to the back axle. In the case of electrically driven cars, the motive power is obtained from accumulators contained in a box placed underneath the car-frame, so that the accumulators can be easily taken out, and changed, or re-charged. The electric motor itself, is well adapted for the required electrical energy to drive the omnibus—there is no change-gear necessary. The accumulators supply the electro-motive power to the motors fitted either to each of the back wheels, or by using one motor to drive the back wheels by means of differential gear. The petrol motor is at present leading, probably by its greater cheapness in fuel than steam and electric power; but it also seems that steam will gradually prove superior to petrol, as the economical working of steam is progressing and the price of petrol most probably increasing. Electric power may follow.

The motor-omnibus service has particularly proved, as also the tramway service to some extent, that they cause the evolution of intense and injurious dust clouds; while the petrol motor-buses cause considerable noise, through the gearing and chain-drives, and infect the air by the discharge of the burnt petroleum vapours, often unnecessarily, as they could easily be discharged into a condenser. Passengers are often exposed to severe shocks through the heavy vibrations in running; foot-passengers, especially children and women, are often exposed to fatal accidents by deliberate reckless, careless, and too rapid

driving. The motor-buses also deteriorate, if they do not destroy, the roads: at any rate they help to do it. In reference to the injury of the roads and the production of the enormous dust-waves, according to the papers of Dr. Hele-Shaw and Mr. A. J. Metcalf, great efforts are made by the road engineers to remedy these evils. The Roads Improvement Association carried out some practical experimental tests to determine the best preparation of tar for application to the roads, and of the best tar-spreading machine. Prizes of 50 to 100 guineas and gold and silver medals were awarded after the trials, the system of one of the borough engineers being considered the best at present, and was well recommended in a paper, which was recently published in this Society's Transactions.

Having now so far briefly reviewed the steam-tram service on rails and the motor-omnibus service on roads, the writer will proceed with a few notes on electric traction.

The motive power for this kind of traction is electricity, produced by the electric power-producing machine called the dynamo. This machine is composed essentially of magnets, between the circular pole-pieces of which a circular iron ring, called the armature core, is put in motion. Insulated copper wire is wound over this armature core, as also over the magnets. The armature core, with its coils moving between a strong magnetic field, generates an electric current, thus producing motive power. According to the amount required, a number of dynamo machines are installed in a central station, in which boilers and engines are erected to set the dynamos in motion. The electric current or motive power thus produced is transmitted by means of cables to the motor or receiver of the electric current, which is constructed on the same principle as the dynamo or generator, and effects the mechanical work. The motor, which is fitted to each axle of an electrically-driven car, is connected with the wheels by means of gearing for the purpose of propulsion. To transmit the current from the dynamo to the motors fixed to the car, a complete circuit must be established from the dynamo to the car, and from there back to the dynamo at the station. The electric current being led from the dynamo or generator to the car by the positive conductor, while the return current is led to the dynamo by the negative conductor.

To convey the electric current from the dynamo of the supply station to the motor at the car several methods have come into use, by overhead wires, underground conduits, or surface contact. The system mostly used is the overhead installation, by which the electric current is led from the dynamo to the overhead wire, from there to the trolley, which

is fixed to the top of the movable beam installed at the centre of the roof of the car. This trolley runs along on the under side of the top wire. From there part of the current passes down through the trolley and beam to the motors of the car, and when its work is done there, it goes through the car-wheels on to the rails, and from there back to the dynamo at the supply station. The overhead positive wire is supported over the centre of the permanent way by means of cross-wires and the necessary attachments to poles of the required height placed at the sides of the street; or by means of centre poles with right and left cross-arms, placed between the two tracks; or, in case of single lines only, by side poles with brackets. Fuller description of this system, as well as of the underground and surface contact system, will be found in the different engineering papers. To deal with them is not the purpose of this paper. The author had, however, opportunity to see the construction and working of the Falkirk Electric Tramways near Glasgow. A most interesting part of this line was the construction and electrical working of several swing bridges. During the opening of these the tramcars were, at the same time, by means of a special switch arrangement, automatically stopped at a considerable distance from the bridge, and at the closing of the bridge released and allowed to proceed. Thus there was no danger of a car dropping into the canal.

With electric traction a somewhat new phenomenon appeared in reference to the very rapid wear and tear of the permanent way, on account of the extreme wave motion imparted to the rail, and the grinding down of the head or bulb of the rail. The real and proved cause of the wearing down of the rail seems not to have been found out yet; it is questioned whether it is an electrolytic effect on the steel from the current affecting the rail structure, or whether it is the continued hammering of the wheels, especially at the joints. Further, it seems that no clearance need be left between the rail-ends at the joints, as it is assumed that the strains and stresses, through the variation of temperature and the pressure and blows from the car-wheels, are taken up by the foundations of concrete or sleepers, or both, by the fish-plates and the intermediate cross-ties of the rails—in fact, of the whole permanent-way structure. The writer thinks that even in the case of good, solid, well-laid concrete foundations or sleepers, it is preferable to allow some clearance at the rail joints to give the whole system greater elasticity, instead of reducing all strains to side-stresses.

For the maintenance of the over-head wire system tower wagons for horse or motor traction are used. The latter seem to be now more in use. They are required to allow workmen to

do such repairs to the wires, frogs, poles, etc., as they find necessary, at the height of the wires above the ground level. The tower is often of telescopic type and is raised and lowered, either by hand or machinery. The top platform is either stationary or revolving. For the maintenance of the permanent way and especially for the economical working of electric traction, a clean groove free of mud, and a clean rail-surface, are most essential to obtain an absolute contact between the wheel and the rail for the conduction of the electric current. But even for steam traction clean rails were considered necessary. Some time ago the writer experimented with a rail-cleaner constructed by him, working the same on some horse tramcars at Reading during two most severe winters. His dynamometrical experiments were carried out on two horse-cars fully loaded, weighing on an average 3 tons. One car in its regular service started with the rail-cleaner down clearing the muddy rails, while a second car, also in its regular service, followed 10 minutes after, also with the rail-cleaner down. The dynamometer showed that the tractional resistance on the cleaned rails was 25 to 35 lb. less than of the first car with muddy rails, so that the tractional resistance was on the average 12 to 15 lb. per ton on the clean rails with a horse-car. But the electric service continued without any rail-cleaning apparatus until later where water-carts were introduced. Such water-carts are used by some of the municipal corporations as constructed by Messrs. Mountain and Gibson and other manufacturers of electric tramway appliances. These cars are electrically driven, and may be either brush-sweeping cars or tramway watering cars. These water-carts carry tanks of a capacity from 500 to 1000 and 1500 gallons. From the writer's experiments, he finds that a cleaner clears the groove and cleans the surface of the rail more effectually than a spray of water can do. He finds that upon dirt rammed by the wheels into the groove water has no great effect. On more freshly filled mud or dust the water softens it, but it is the flange of the wheel which is then scooping it out, not the water. It seems to him that watering or sprinkling is useful to lay the dust of the road and rails, but it does not carry it away. A water-cart waters about 20 to 25 miles per day, using on the average 700 gallons per mile of single line, inclusive of the road side. The water-carts seem more useful to water the road and the permanent way than for the specific purpose of cleaning the rails. To mention only one example as proof of the importance of obtaining clean rails and dustless roads, the tramway department of the Ilford Tramway Company use a combined water-car and sweeper with a water-tank of 1800 gallons capacity. The car

is provided with two electrically driven rotary brushes, one at each end. Also with a sprinkler-pipe at each end for watering the grooves, and with groove-cleaners and special track brushes. As to cost, it has been stated before by the engineer of one of the largest tramway companies at the time, that to clear the grooves and to keep the permanent way, that is the rails, in clean condition, using hand labour and horse water-carts, costs the company about £1700 per annum, independent of the extra cost of the winter period, which works out to about 2*s*. to 2*s*. 6*d*. per mile of single line. The occasional clearing of snow according to the severity of the winter is reckoned as extra expense, as it does not occur regularly. The cost of working the electrical water-car comes to about 18*s*. to 20*s*. per day for an average of 25 miles of road and permanent way watering as mentioned above. The clearing of the grooves, inclusive of the cleaning of the rail surface by the author's rail-cleaner fixed to the horse cars, came to about 4*d*. per day. Assuming breakages, which, however, did not occur, this may be increased to 6*d*. per day.

In comparison with the electric tramways the same tractive power on common roads, as represented by the electrical omnibus, called the electrobus, has also to be noticed. This kind of traction has the advantage over the petrol omnibus of freedom from noise, vibration, and asphyxiating vapours of burnt gases, and there are not the usual breakdowns on the road as with the petrol buses. The electric current as the tractive power for these omnibuses is derived from secondary batteries or accumulators, carried on the vehicles. There is necessarily a large storage warehouse or accumulator gallery for the supply and maintenance of the batteries, to which the battery makers attend for the traction company at the rate of 2*d*. per mile, while the company supplies the charging current. It is stated that recharging the accumulator batteries, or replacing the same with fresh ones, is only required after a run of about 30 to 40 miles. The weight of the omnibus without the batteries is about 3½ tons, and with the batteries and fully loaded with 34 passengers about 7 tons. The cost of managing and maintaining an electrobus is stated to be lower than other systems, as there is on this omnibus no change-speed gear-box as in the petrol bus, and no generator furnace and engine as in the steam vehicle. It is further stated that the expense of the breakdowns of the other buses amounts to a loss of 2¼*d*. per mile when running. It is further calculated that one day's stoppage is not only a loss of revenue of 1*s*. per mile, but the proportion of standing charges is 18*s*. 2*d*., which together with the revenue amounts to a loss of 5*l*. 18*s*. 2*d*. An hour's delay

in the streets means at least a loss of 12*s.* 8*d.* According to Colonel Crompton's paper the working cost of motor omnibuses in London in September 1906 came to 10·272*d.* per mile—the author, however, was informed by drivers that it came nearer to 12*d.* per mile—while steam-traction, according to Mr. Clarkson's paper, came in the same year to 8·87*d.*, say, in round figures, 9*d.* The cost of the electrobus operation, including depreciation, is, as stated by this company, under 9*d.* per mile.

Having thus noticed the start of the steam-omnibus service on common roads, followed by the steam tramway service on rails, then the electric-tramway traction followed by electric-omnibus traction on common roads, with some few appliances for the maintenance of these services, it remains to mention an important accessory to the tramcar service, namely the life-saving apparatus or lifeguard, fitted to the tramcars by order of the Board of Trade.

Fig. 23 represents the American lifeguard, known as the Hipwood-Barrett fender.

Fig. 24 shows the English Wilson and Bennett design.

Comparative Conclusions in reference to the Different Kinds of Tractive Powers in Use and the Methods of their Application on Tramways and Common Roads.

The motive powers used before, and those in use now, are steam, compressed air, gas, and lately electricity; the petroleum vapours for petrol omnibuses the writer classifies under gas, while the cable-trams can be worked by any of the powers. The largest experience we have is of steam, being the first of the powers which came in extended use, and developed in manifold industrial applications, extending over more than half a century. It is steam, in comparison to all other motive powers, which has revolutionised the commerce and the industries of the world, and which is still the prime mover in the traction service of all the great railways and of the steamers crossing the seas to the four corners of the world; and yet steam has been generally replaced by electricity in tram service. The writer, however, maintains, according to his experience, that at the time, steam has never had the chance of being used to its best advantage; steam service has not been given the time to develop its own improvements, as time, patience and opportunity are now given to petroleum service on public roads, or to the electric installations which are sometimes defective and not quite reliable. The writer maintains that such defects

in the steam-service as the emission of smoke, or exhaust-steam, were very slight, and soon removed by the use of coke as fuel instead of coal, while the condensing arrangements were greatly perfected. Never have local authorities shown so much favour to steam traction on tramways as they show now to all other methods, permitting, in the case of continually occurring breakdowns of the petrol-motor traction, repairs to be made in the street, impeding all other vehicular and foot passenger traffic. The writer, in his steam-tramway service, when a breakdown accidentally occurred, had to roll the locomotive off the line by laying plates over the permanent way, and to transport it immediately into the repairing-shop. No repairs in the street were allowed, and a fine was imposed at any time an engine showed some exhaust steam.

Comparing the electric installations required for the establishment of the power-house for the production of electric motive power for either the overhead or underground or surface contact systems with the installation required for the production of steam, and then the steam-tram construction with the comparative simplicity of the construction of the engine itself, the writer maintains, from his own practical experience of the steam tram service, that the same can be conducted as efficiently, and probably with less cost, at any rate, not with greater cost, than the electric, or the service of any other power. Steam motor-omnibuses, in comparison to petrol motor and electric omnibuses, have proved to be less costly, or at least not more costly, than the two competitors; the author therefore feels convinced that steam will continue to hold its own.

The author has used, in the compilation of this paper, information drawn from the articles by himself and other writers in the engineering papers, as: "The Engineer," "Engineering," "Engineering Times," "The Tramway and Railway World," D. K. Clark's book on tramways, and papers by Colonel Crompton and Mr. Clarkson.

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Figs. 1 to 13 and 21 from "The Engineer."

Figs. 14 to 19 from "Engineering."

Fig. 24 from "The Tramway and Railway World."

DISCUSSION.

The President, in moving a vote of thanks to the author for his paper, said he would like to point out that it was some years since they had had this subject before them. The last

occasion seemed to be in 1874, so that there was much leeway to make up. The author had brought before them some interesting facts, historical and otherwise, going back to that good old friend of theirs, Cugnot, whose engine produced such an effect on the traffic of Paris. He seemed to have foreshadowed the effect that was produced at the present time by some of the motors that were running, but the authors of those motors were not treated in the same way that Cugnot was. With regard to cable tramways, their past-President, Mr. Colam, read a paper to the Society upon the subject as long ago as 1885, and he alluded to it again in his presidential address five or six years later, when he also mentioned the "Spot" system of tramways, which, under another name had recently been on trial. Mr. Conradi himself read a paper on the Cleaning of Tramway and other Rails in 1893. As a school-boy in London, he (the President), saw Train's tramways put down and subsequently taken up again. In these the rail was Z-shaped, with a drop of three-quarters of an inch to an inch, and serious effects were produced upon the wheels of ordinary vehicles. Along the same road they now had the modern tramway system. He felt that Mr. Conradi had done well to bring the subject before them, to give them an opportunity of ventilating their opinions on the matters which he had introduced. The figures which he had also given would be useful for comparison. He would ask the meeting to join with him in the vote of thanks.

Mr. Holroyd Smith said that the paper covered such a very, very wide area that it was exceedingly difficult to decide what particular topics to deal with. There was a saying that the worst listener a preacher could have was another parson, because he naturally wanted to treat the text in a different way from that in which the speaker did, and he (Mr. Holroyd Smith) felt himself in the position of the other parson. He was afraid that he could not speak in an altogether complimentary way of the paper. He took exception to the title. He regretted that a gentleman of Mr. Corradi's experience should not have given a more analytical account of his own personal experience of the various systems of tramways he seemed to have investigated. If he had done so, they might possibly have learnt something. Perhaps it did not sound very affable, but he failed to learn anything, although he had carefully read the paper through. To whom was it addressed? Was it addressed to students who knew nothing at all about the subject? If so, then he should have to take exception to the manner of imparting the knowledge. Was it addressed to engineers who were experienced in the work? If it was, it became a species of weariness, because it did not open their eyes to any further knowledge on the subject.

They had a short reference to the Wilkinson steam engine. He had had the privilege of riding upon that and upon many other of the cars mentioned in the paper. Wilkinson's engine was remarkable in that the transmission from the engine to the road wheels was by a worm gearing, as the paper mentioned. There was a very peculiar feature about that worm gearing which puzzled him considerably, and that was that the teeth of the worm wheel and the worm were made very deep, and a certain springiness of the tram engine was compensated for by the worm and wheel going more or less into mesh. A more unmechanical method of applying worm gearing it seemed difficult to conceive, yet the Wilkinson engine showed that the thing could be worked. That was a very great deal, but he had never been able to obtain any data with regard to the mechanical efficiency of the engine. He thought that probably the partial success of the worm gearing of Wilkinson's engine was due to the very roughness of its conception. Wilkinson used worm gearing of a very coarse pitch, and if worm gearing was to be effective for transmission purposes, a coarse pitch must obviously be used. The demonstration of the efficiency of worm gearing and the possibility of it was, he thought, due to himself. He showed that he obtained with an electric motor and worm gearing a combined efficiency of 85 per cent. Measuring the electric power absorbed by the motor and the output given by the dynamic test on the axle of the car, he had a total efficiency of 85 per cent. in face of the fact that the then text books declared that the loss with regard to worm gearing was 75 per cent.

The author had described the swing bridges used for the Falkirk electric tramways near Glasgow. He did not know the date of the construction of those tramways, but it was after he had worked out and submitted to engineers in France a scheme for dealing in a similar way with some tramways to pass over one of the navigable channels on the outskirts of Paris.

A very interesting discussion could be raised on the wave motion of the rails, and as to the suggestion that the wear and tear of electric tramways was due to electrolytic effect. If he understood correctly the use of the term "electrolytic," he could not quite agree with the author. The wear and tear was more rapid with electric tramways than with others, although the load upon each wheel was less than in some steam tramways previously referred to. As it could not be due to the hammering action, it must arise from some other action inherent in, and due to the use of, electricity. He would not state a definite opinion upon the subject, but would simply offer a suggestion. Upon a dry day when there was dust over the road they would notice a sparking and cracking taking place under the wheels. There was always dust on the road, and

that dust made a partial gap between the wheel itself and the rail upon which it ran, and the electric energy caused sparking across the gap. Upon careful examination they would find little burnings or pit holes in the wheels and in the rails. The effect was the same as when passing electric current between two iron bars. They would burn away. The surface was disintegrated by means of the frequent, very miniature, breaks of the electric current.

He thoroughly endorsed what the writer had said with regard to the necessity of keeping the rails clean. A little rail cleaner, the invention of Mr. Hipwell, came under his (the speaker's) notice some years ago. It was a pretty idea, but through the inventor's want of influence in the tramway world and through his want of capital they never heard very much of it. There was a water cart, and just immediately over the rail, lying longitudinally, there were two spouts coming down in the same way as the two jets of gas come out of the burner for acetylene gas. Those two jets meeting together in the groove practically sprayed the dirt up, and threw it sideways. He saw the apparatus tested on more than one occasion, and it cleaned the rails perfectly. He doubted the correctness of the statement "On more freshly filled mud or dust the water softens it, but it is the flange of the wheel which then scoops it out, not the water." The flange of the wheel did not go to the bottom of the groove, so how could it clean out the bottom of the groove? There was a statement with regard to the cost of cleaning with "groove cleaners" which was a little bit indefinite. He took it that the 1700*l.* worked out to 2*s.* 6*d.* per car-mile exclusive of the extra cost of the winter period, but the sentence did not so state. It might be read in two ways.

He must join issue on two more points. The author said that steam-service had not been given a fair trial. Surely, as it was more than a century since steam was first used, and it was only thirty years ago that they had the first practical electric tramway, steam had had a better chance, and yet the author wanted to make out that the reason why the use of steam was diminishing was because it had not had a fair opportunity.

Then the author almost stated, that while with petrol motors they had objectionable asphyxiating gases, steam was free from them, but everybody knew you could not have combustion without the products of combustion, and the products of combustion were poisonous. Was anything more likely to produce asphyxiation than the burning of coke? The gases were given into the air just as the exhaust from the petrol tank was given into the air.

On the question of the relative value of different systems,

no positive statement could be made unless the conditions under which they worked were taken into consideration. By all means have steam, or storage batteries, or petrol motors for a country line with a service of only every quarter of an hour, but such things were not to be considered for a town with a service of every two and a half minutes. They must have electric power for a service of that kind. He thought that the cable system was by this time an "exploded idea," because, although they had it in America, yet on the authority of Lowell "they didn't know everything down in Judee." He believed that one of his inventions connected with the question of pulleys and the method of gripping was worked on the Highgate line from its very commencement.

He was very pleased to hear the statement about the electro-bus. If the total cost came down to 9*d.* per car-mile, then there seemed to be hope of the separate unit system of electricity by means of accumulators being at last a success. If more skilled engineers would apply their brains to the details of petrol motor-buses they would be made to pay better, and to give a better service than even electric tramcars could give, because there was now very much less tractive resistance on the roads than there used to be. The step tramway of which the President had spoken was largely used, he believed, in some American towns at the present time. He would like to find some statistics which would give a comparison of the relative costs of running a petrol car over a properly paved road and running electro-buses on the same roads, but every item must be included. He had frequently asked, when statements had been made as to the cost per car-mile of one particular system, "What do you include in the cost of working?" Until that question was answered, comparisons between one cost and another could not be properly dealt with.

Mr. E. Benedict said he would only touch upon the question of the road. To his idea a tramroad was about the most absurd thing that could possibly be conceived because it went straight against all the principles which he, as a railway engineer, had been taught from his youth upwards. There had been a steadily increasing weight at a steadily increasing speed on a steadily increasing solid bed. If they had a train with 25 tons on the axle going at the rate of 60 or 80 miles an hour they would be nowhere, as the old coachman said. It seemed to him that tramways were following in the footsteps of the early railways which had coaches stuck upon four wheels and cast iron rails with stone sets. All that had to be gradually done away with and something quite fresh had to be started. Tramway people considered that they started where railway people had

left off, but they did not, as the conditions were quite different. They ought to have made a new start altogether. Automobile people wanted an iron road. That was their ideal, but his idea was that an indiarubber road would be better still, and that they would come to that in time when indiarubber was cheaper. Previous speakers had said that the objection to many of the roads in America was the sudden ridge.

Several years ago he had proposed the arrangement shown in Fig. 25. Two such metal plates were provided, the small groove in each being for the flanges of the tramcar wheels, while the broad top surface was for the ordinary traffic. The curved dotted line W was to show how the wear of the sets at the sides would take place without forming a sudden step.

They would see the advantage of the wavy surface of the plate road. If the people who owned the trams also had to maintain the roads, it would be better; but unfortunately one authority or one committee had the trams and another the roads, and there was constant fighting. Everything that went on the tramways was an advantage to the roads. The diagram showed a longitudinal baulk of timber into which the plate was fixed with sets on each side. A kind of wedge was formed which kept the sets tight, and it seemed to him that something similar to what he had indicated was what was wanted. The knocking to pieces was due to the rigidity of the track. Roads should be wide enough to take two vehicles abreast on each side of the tramway, and where they were not tramways should not be allowed. On roads of a sufficient width tramcars should take the middle of the road, and pass like ships at sea—port to port. A little reflection would show the many advantages of this.

Mr. R. W. A. Brewer said that he agreed with Mr. Holroyd Smith's remarks with regard to the conclusions and with his statement that steam had had a fair chance. He (the speaker) had brought a few small prints which might interest the meeting. It seemed to him that they would see from them that the use of steam as a method of traction upon roads had not very much improved during the long time in which it had been exploited. Importance must be attached to the conditions under which a system had to work. They could not go straight ahead and say whether one system was better than another unless they knew exactly the local conditions.

He had tabulated a few of the advantages and disadvantages of tramways and other systems of traction. The advantages of a motor-bus service which did not run upon any restricted track were, first, the possibility of greater speed from point to point owing to the track not being restricted and the bus being allowed to take any opening in the traffic. Secondly, the service could

be varied from one route to another, enabling the most profitable to be worked at any particular time of day or season of the year. Thirdly, there was the absence of rails in the roadway, which rails caused great inconvenience to other traffic and were a continual source of obstruction during times of laying and times of repair. Another advantage of the motor-bus, or of any system that did not confine itself to rails, was that the vehicle could draw up at the kerb to take up passengers or to allow them to alight. The minimum of risk to the passengers was involved, and other traffic was allowed to pass along the road without being severely obstructed. Tramways ought never to have been allowed in a town like Brentford, because they took up the whole of the road, and every time that a tram stopped the whole of the traffic was disorganised.

The question of capital expenditure and the return upon the outlay should be very carefully considered. Capital expenditure on a tramway system was very much greater than on any system which did not run upon rails.

The disadvantages of a motor-bus system were, higher working cost and depreciation of rolling stock, and the possibilities of noise when it was allowed to get into a defective condition. That disadvantage was rather more apparent than real, because a motor-bus could run quite quietly if it was kept in decent order, while a tramway service was not always so quiet as tramway enthusiasts tried to make people believe. The trams on the Uxbridge Road were far noisier than any motor-bus that ever ran in the streets of London. Another disadvantage of the motor-bus, and a very serious one, was the risk of skidding, but it was not always attributable to the motor-bus itself. The roads and the restrictions under which a motor-bus is run had a good deal to do with it. They were not allowed to adopt any non-skidding device which would have a really beneficial effect on a greasy road. The road surface had a very large bearing on the cost of working any system which did not run on rails.

With regard to reliability, an electric tram was more reliable than a motor-bus. The author had said that motor-buses were continually breaking down, and that was true with regard to some of the older ones, but they must consider the enormous number of miles that the bus ran and the pressure of the service. The buses ran from early morning to late at night with no possibility of any general overhaul. That spoke well for them, considering the little delays that actually occurred.

The question of vibration was rather a large one.

Coming to the advantages of a tramway, they had low cost of operation, cleanliness and ease of running, absence of smell, reversibility of the car, and improvement of the road surface

under certain conditions. If the tramway company re-laid the track, as was usual, they got a better road surface where the tramway occurred, but very often an abominable surface on either side of the track about a foot away from the rails. That was a great source of danger, particularly where the sides of the road were made of macadam.

The disadvantages of a tramway were, first, the obstruction of other traffic, and particularly was that the case when central standards were employed. A tramway had an enormous effect on the carrying capacity of the road. On the portion of the tramway which ran through Ealing or Ilford, where there were central standards, the carrying capacity was reduced to about one-fourth, and the trams themselves suffered very considerable delays, owing to the other vehicles on the track, particularly where the road was narrow and there was not sufficient room between the near side rail and the footpath for another vehicle to stand.

With regard to cost, out of 71 tramway corporation systems throughout the country only six received an average of 1*s.* per car-mile, and only about 30 were paying their working expenses and interest on the capital outlay. The total cost of running, which included the working expenses, was 6·34*d.* per car-mile, and the interest on the capital, charges, sinking fund, etc., was 3·62*d.*, making a total of 9·96*d.* per car-mile. That represented the minimum remunerative revenue. The actual mean revenue obtained was 10·52*d.* per car-mile, which left an average net profit of about ½*d.* per car-mile for the whole. The working expenses for a fleet of petrol omnibuses came to a shade under 9½*d.* per bus-mile, and the depreciation to 1·407*d.* per bus-mile. The total minimum remunerative revenue was 11·7*d.* per bus-mile. More than that had to be earned, if the system was really to pay at all. The cost was little more than 1*d.* a bus-mile greater than the cost of working an electric tramway, but the capital expenditure in the case of the tramway, taking a certain section, was 88,000*l.*, and in the case of the motor bus 25,000*l.*, for the same return. The difference in the figures went towards depreciation. The cost of working a fleet of 100 electro-buses came to 9·5*d.* per car-mile. This included maintenance and depreciation. Tyre maintenance was 2*d.* per car-mile, and battery maintenance also 2*d.* He was taking actual figures.

He had taken a great interest in the subject of self-propelled vehicles on rails, apart from steam engines, and he had brought with him some outline drawings of a design which he had made to suit certain very difficult conditions. The vehicles were driven by the ordinary spirit which was obtainable in England

at the present time. Each was designed to draw a small train of about 18 tons on a light railway. They had to travel at the rate of 15 kilometres an hour, and draw the train up a gradient of 1 in 25. The temperatures under which they had to work were between the maximum extremes anywhere in Europe, between several degrees below zero on the Fahrenheit scale and 120 or 130 degrees above it. They could not run a steam locomotive under the conditions, but there were many conditions under which such a locomotive could be run. He thought that the author was rather biassed in favour of steam for all purposes, and he did not agree with him. He was using steam traction for a system which was entirely different, and was more suited to it than to petrol. In France and Germany locomotives of a similar type to that to which he had referred had been running for a number of years with very great satisfaction. They were run with petroleum spirit and they worked at the rate of 1·37*d.* per ton-mile. That was the total cost of working, including everything. They were used in hauling from mines. On the same work rope traction came out at 1·65*d.* per ton-mile, and horse traction at from 2*d.* to 3·4*d.*

Mr. Delves-Broughton said that he thought engineers had neglected steam, and that it had not had quite a fair trial yet. The only men who had ever really done anything with regard to it were Mr. Clarkson and M. Serpollet. In the discussion on Mr. Clarkson's paper, read at the Institute of Mechanical Engineers, the remark was made that, up to the advent of the flash boiler, engineers had been working with hot water—not steam.

He (the speaker) quite agreed that fumes from a chimney were equally as objectionable as petrol fumes, but petrol fumes were down on the ground, whereas chimney fumes were usually in the air. He did not know why the exhaust was placed in the position universally adopted in the case of petrol motors.

With regard to road traction, he thought that instead of looking for a new road the thing to be looked for was a new tyre. Rubber was altogether too expensive and too slippery in damp weather. If a tyre which would grip the road a little better could be devised, a great step would be taken towards reducing the cost of road traction.

Mr. E. J. Silcock said that the most important part of the paper was in the conclusions at the end, and it seemed to him that there was room for a great deal of difference of opinion with regard to those conclusions. In his view the history of the past with reference to steam tramcars and steam motors was a pretty clear indication of what was going to happen in future. He did not believe that steam would ever be applied to tramcars

again. He had had the misfortune to live in Leeds, where in years gone by, there were a number of steam trams, but he was glad to say that that town was the first in England to have an electric overhead tramway. He thought that the steam tram was the worst abomination that could be put upon a road, and was offensive alike to the senses of sight, smell, and hearing. In his opinion the real reason why steam could not be properly applied to tramcars was the small size of the unit. Progress in the steam engine had always been in the direction of increasing the sizes of the units, and that was the reason why electric traction was so successful: by using large steam generators, with electric current as a means of distribution of power, high economy had been effected.

He must join issue with the author on the question of municipalisation. Unless the municipality had control of the tramway system there must always be friction between the tramway authority and the highway authority. Even with municipally owned tramways where there were separate tramway and highway committees, there was sometimes difficulty owing to dual control, and it was better to have one committee dealing with both questions.

With regard to cost of running per car-mile, they must take everything into account, and remember that a tramcar usually carried a good many more passengers than an omnibus. Again, a large part of the cost of road maintenance was included in the running cost of tramways, whilst, in the case of the omnibus, these charges were thrown on the community, so that the two were not comparable.

Mr. A. W. Galbraith said that the question of corrugation was a serious one. The explanation given by Mr. Holroyd Smith was the most ingenious that he had yet heard, but there were facts in connection with corrugation which that explanation did not entirely cover. He had examined corrugated rails in many towns, and he had found that the wave axes were not normal to the longitudinal axes of the rails. Certain cars seemed to have a greater effect in producing the corrugation than others had, and there was no doubt that oscillation had a good deal of bearing on the subject. If they accepted Mr. Holroyd Smith's theory they would expect to see corrugation over the whole of the system, but that did not occur.

The President had referred to the "spot" system, which he took to mean the surface contact system. The only successful system of the kind was the Lorain that had been introduced at Wolverhampton. It was very ingenious, but very costly. The entire cost per mile wholly equipped was about 16,600*l.* against 14,000*l.* for the overhead or trolley system. A great advantage

was that you could run directly on to the overhead system. There were advantages and disadvantages connected with all these systems, but in consequence of the lateness of the hour there would not be time enough to deal with them then.

Mr. Holroyd Smith said that in his previous remarks, to which Mr. Galbraith had referred, he was dealing with the extra wear which occurred when electricity was used. He spoke of disintegration of the surface. He quite agreed with what Mr. Galbraith had said with regard to wave action.

Mr. J. S. Warner said that he came to the meeting to be instructed, and in a large measure he had been instructed. He was at the recent Congress at Munich as a member of the International Tramways Union. The principal tramway managers and engineers in Europe were present, and almost every subject which worries the average tramway manager or engineer, including the vexed question of corrugation, was discussed.

In his opinion it was altogether a mistake to deal with rail vehicles and vehicles which did not run on rails in one paper. He thought that it was wrong to omit any reference whatever to the sections of rails employed. There were only about half a dozen types.

He was very interested to note that in Budapest they were using a rail for a wheel with a central flange, a tread on each side of the flange. He thought that at Hull they used that type of wheel.

In Philadelphia they had a rail with the tread overhanging the web. There was a great amount of corrugation which was traced to the weight of the car thus being carried eccentrically. The rail, as the car passed along, was depressed, and the web deflected sideways, and very serious vibration was set up. They stiffened the web with plates and got rid entirely of the corrugation.

Even when riding in the finest railway carriage in existence one's body was subjected to vibration and oscillated the whole time. A friend of his who was not an engineer said that he could ride in his automobile from London to Edinburgh with much less fatigue than he could by one of the best trains. The fact was that owing to the modern principle of automobiles, motor-car tyres got a better grip of the track than rail vehicles did.

The obvious remedy for corrugation was to place the web directly under where the weight was imposed on the rail, and he believed that that was done in Philadelphia, but the British Standards Association had the web on one side.

With regard to self-propelled road vehicles, the principal difficulty with which they had to contend was the vertical varia-

tion of the road. There was very little difficulty in that respect on a tramroad. There was difficulty from transverse irregularity and that applied to tramways and to railways.

With regard to the cost per car-mile, 6*d*. was the figure generally taken. Some two years ago the managing director of the Brussels tramways, who had made all the inquiries that he could in London as to the cost of motor-buses, said that he obtained the figure of 9*d*., put on 2*d*. and called it 11*d*.

REPLY.

Mr. Conradi, in reply, said that he was obliged to Mr. Holroyd Smith for his observations, to which he would reply more fully in writing. He could not enter into greater detail on the subject with which he had dealt, otherwise the paper would have been too long. His manuscript was originally much more complete, but the Secretary had advised him to write simply an extract with regard to the practical points to be brought forward.

He had very often heard from electrical engineers that electricity was the only power upon which reliance could be placed for traction, but as he was a strong advocate for the use of steam he thought it necessary to say something in its defence, and to remind the electrical engineer that steam, as a prime mover, is used to move the dynamo first, to enable it to produce the electric energy required, as secondary motive power. He thought they would find, as he had stated, that steam trams had not had an opportunity of developing all the advantages which they could offer, while electrical and petroleum motors had. That the application of steam had developed, and that great improvements had been made, they had actual proof of in Mr. Clarkson's steam car, which was now on the road, and which used superheated steam, and also in the invention of M. Serpollet, which had been greatly improved of late.

With reference to the Wilkinson engine, notwithstanding the rough construction of the worm gearing, it worked exceedingly well, and it was perfectly reliable.

As to rail-cleaning, he did not mean that the flange scooped the mud out, but it squeezed the mud out when it had been softened by water.

He would like to read a statement made in the report of the Royal Commission on London Traffic: "Tramways will continue to be the most efficient and the cheapest means of street conveyance, and we cannot recommend the postponement of tramway extensions in London, on the ground of a possible

prospect of the supersession of tramways by motor omnibuses." That was his answer. He was naturally in favour of tramways as against motor omnibuses, whether the trams were driven by electricity, steam, or other power. His experience told him that tramways were of greater public benefit than motor buses could ever be. He thought that motor buses would work well as feeders to small branch lines and to tramways, and they should be used on roads where there was not sufficient room for tramways, and where widening would cost too much.

Private companies could not often spend the necessary money to keep their plant in perfect condition. With regard to steam tramways, it was necessary that there should be at the depot a night-shift to inspect the engines, and do all the small repairs which were necessary. In many cases companies were very much hampered with regard to money, and very much pressed by local authorities.

With regard to his paper, he thought that it was interesting to bring before engineers an outline of all that had been done from the beginning up to the present time, mentioning all the important points of interest. He hoped that the paper would bring the subjects to the minds of engineers who would consider them, and carry out their practical application.

In further reply to Mr. Holroyd Smith's observations as to the Wilkinson Steam Tram Engine using worm and wheel gearing drive, the author draws attention to the important fact that the steel worm keyed on the crank shaft geared into a phosphor-bronze wheel keyed to the driving axle, and ran constantly in an oil-bath to reduce friction, wear and tear, and the effect of the roughness of the gearing.

In reference to the electrical working of the swing bridges on the Falkirk Tramways, a full description of the installation of the winches, and their connection with the turning of the bridges, is given by the contractors, Messrs. Bruce, Peebles, and Co., in "The Tramway and Railway World," December 7, 1905.

As to Mr. R. W. A. Brewer's observation that steam trams had had their opportunity to produce a good service, but were less successful than the petrol motor omnibus was already, or would be in time, the author begs leave to remind him, that the steam trams were then worked by private companies, which, by order of the municipalities or local authorities, had not only to maintain the steam-service with the permanent way, but part of the road also—consequently they were charged with a heavy outlay for the maintenance of the roads, and were greatly hampered financially, in working the engine service as efficiently as it could have been done if free of such heavy extra charges.

There were no daily breakdowns, no discharge of burnt

vapours, no dropping of oil and grease, no severe shocks, heavy dust clouds or vibrations, and less danger of fatal accidents.

The same reply nearly applies to the observations of Mr. E. J. Silcock who said that the steam-engines and cars did not look sufficiently clean. As at present, the tramway traction services are mostly carried out with the assistance of the ratepayers' money, and with plenty of it there would be no difficulty in establishing a perfect steam tram service. The engines of Merryweather and Sons, the different types of Hughes, Wilkinson, Brown of Winterthur, and others, were all designs of small locomotives of the usual practical and neat design, following the general principle of locomotives, but applied to their special purpose.

In the selection of the most suitable form of tractive power which should be applied, according to the author's experience, the following matters would have to be considered: (1) Cost of installation, (2) cost of working, (3) density of traffic, (4) absence of smell, dust, and danger.

As to the cost of installation, the most moderate would be steam, then follow gas, electric overhead wire system, electric surface contact, compressed air, electric accumulator, and electric conduit system.

As to cost of working, in order of cheapness we have: steam tram traction, electric overhead system, steam road motors, electric traction by accumulators, compressed air, and gas.

Concerning the construction of the permanent way and roadway, as mentioned by Mr. Ernest Benedict by means of steel T's grooved for the passage of the flange of the tramcar wheels and supported by sets of hard wood, as shown in the sketch (Fig. 25), the author draws attention to the fact that the cost of such an iron-plated road would be excessive. It would be somewhat similar to the cast-iron block rail laid in Boston, U.S.A., in 1856, weighing about 75 lb. per yard in sets approximately 5 inches long by $2\frac{1}{2}$ inches deep, with a groove for the tramroad. It was, however, also mentioned by Mr. Benedict that all vehicular traffic should pass over the plated road, that right and left of the tramway sets it was intended to have a plate-way inserted over the full width of the road; say similar to the New York step rail of sets 8 inches long by $\frac{3}{4}$ to 1 inch deep. To this must be added, the excavation of the ordinary road, and the laying of the plate-ways, the tram-blocks and the hard wood sets as their support.

In reference to Mr. J. Sutherland-Warner's observations, the author has travelled in first class private motor carriages and in first class railway carriages at different speeds. The railroad is naturally more even than the ordinary road, but what Mr.

Warner calls the transverse rail irregularities, the author understands to mean, that on some parts of the line, and on curves, swaying of the cars is produced, especially at high speeds. This is caused either by insufficient ballast under the sleepers, or by loose bolts in the fish plates and joints, rail chairs working loose, or the carriages not being properly balanced. Further, the gauge of the permanent way may not be quite exact, and in some places the rail may have been pressed inward, in other parts outward, which the platelayer has to rectify. Comparing the surface of the common road, with that of the rail surface, greater irregularities are naturally to be found in the former. The flexibility of the indiarubber tyre and the motor carriage springs have to counterbalance this, by their slight depressions, the indiarubber if of sufficiently elastic material, displacing its own particles, to fill up the hollow over which the car passes. As at the same time the whole weight transported is much smaller than that of the railway carriage, it is possible that the comparatively slight vibrations are sometimes not so much felt, as the displacement of the body of a railway carriage, of so many tons, of which the centrifugal force due to speed is also so much greater.

THE HISTORY OF MECHANICAL TRACTION ON TRAMWAYS AND ROADS
BY H. CONRADI.

PLATE I.

STEAM TRAMWAY CAR

Fig. 1.

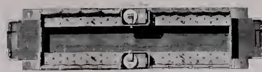


Fig. 2

Fig. 3.

Fig. 4.

STEAM OMNIBUS.

Fig. 5.

Fig. 6

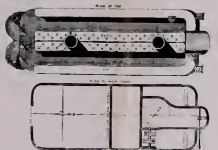


Fig. 7.

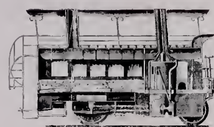


Fig. 8.

Fig. 9.

Fig. 10

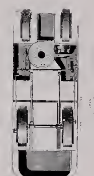


Fig. 11

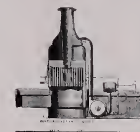


Fig. 12.

Fig. 13

STEAM TRAMCAR (FAIRLIES SYSTEM) FOR NARROW GAUGE TRAMWAYS
DESIGNED BY MR. A. BRUNNER, ENGINEER, BERNE, SWITZERLAND

Fig. 14.

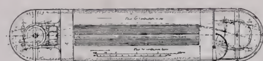


Fig. 15

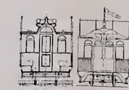


Fig. 16

BRUNNER'S STEAM TRAMCAR; LAUSANNE AND ECHALLENS RAILWAY.
CONSTRUCTED AT THE SWISS LOCOMOTIVE WORKS, WINTERTHUR.

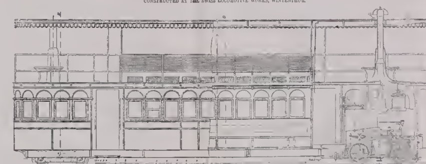


Fig. 17.

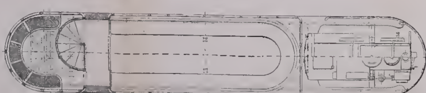


Fig. 18.

BRUNNER'S STEAM TRAMCAR LAUSANNE AND ECHALLENS RAILROAD

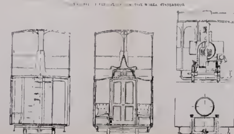


Fig. 19.

SYSTEME CH BROWN WINTERTHUR

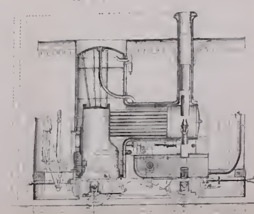
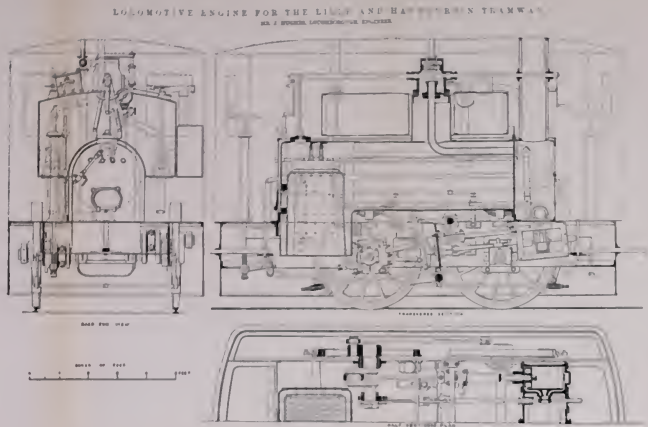
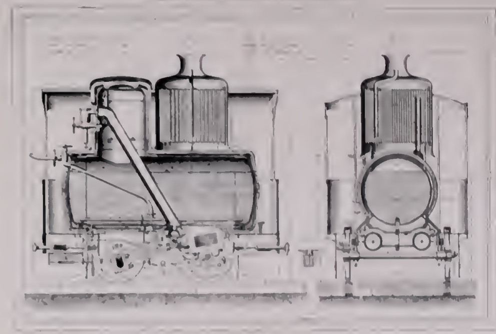


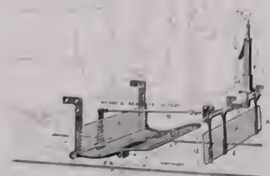
Fig. 20.

THE HISTORY OF MECHANICAL TRACTION ON TRAMWAYS AND ROADS.
BY H. CONRADI.

PLATE II.







THE HISTORY OF MECHANICAL TRACTION ON TRAMWAYS AND ROADS. BY H. CONRADI.

PLATE III.

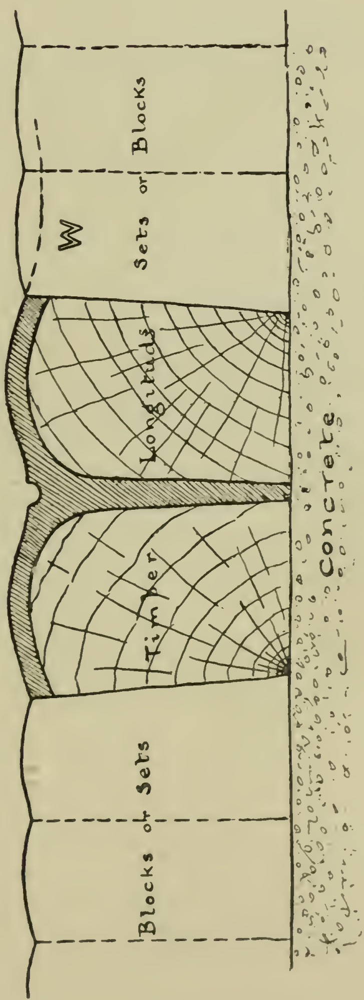


FIG. 25.

November 2nd, 1908.

JOSEPH WILLIAM WILSON, President, in the Chair.

THE FLOW OF LIQUID FUEL THROUGH CARBURETTOR NOZZLES.

By Robert W. A. Brewer, A.M.Inst. C.E., M.I.M.E., M.I.A.E., M.S.E.

The Action of a Carburettor Jet.

In the autumn of 1907 the author placed before this Society some figures showing the results of his experiments upon the evaporation of liquid fuels. He there made certain statements with regard to the viscosity of various fuels and the effect of this viscosity upon the action of the carburettor. Modern practice in carburation of liquid fuel is almost universal in the adoption of some form of jet which more or less disintegrates the fuel in the presence of a current of air. We may take it for a fact that in the portion of the carburettor occupied by the jet only the more volatile fractions of the fuel become vaporised and that the heavier fractions are carried forward through the mixing chamber into the induction pipe in the form of finely divided particles in suspension.

There is a certain amount of difference of opinion amongst automobile engineers as to whether the fuel actually enters the cylinder in the form of a fine spray, but assuming that it does the question is whether a loss of thermal efficiency results owing to the explosive mixture not being homogeneous. A statement has also been made to the effect that fuel in a liquid form entering the cylinder "cracks" and leaves tarry deposits upon the piston. The author's opinion, based upon some considerable experience in the utilisation of liquid fuels, is that there is very little if any loss of thermal efficiency owing to the fuel being only partly vaporised when entering the cylinder of a high speed engine, and that during the compression stroke vaporisation must be practically completed.

Also the author has not found tarry deposits when utilising petrol which has been only partially vaporised, but when certain

paraffins have been mixed in large proportions with the petrol, tarry deposits have been traced.

Soot, however, has occasionally formed owing to an insufficiency of air.

The objects of a jet are twofold:—

(1) To regulate the rate of flow of the fuel to suit the demands of the engine.

(2) To disintegrate the fuel mechanically into the finest particles possible.

Now these particles of liquid must be converted into a gaseous state, and one of the most important and difficult adjustments to be made in connection with carburation is the regulation of the amount of heat which must be supplied to equalise the latent heat of evaporation of the fuel.

In employing the word "equalise" the author wishes to emphasize the several points which have the most important bearing upon the subject: (*a*) The minimum temperature at which the fuel can exist as vapour; (*b*) the time taken to effect the change between carburettor and engine; (*c*) loss due to raising the temperature of the charge above normal. Under the heading (*a*) the subject was discussed by Mr. G. H. Baillie in a recent paper read before the Royal Automobile Club. He says: "The minimum temperature at which it is possible for a fuel to exist as vapour is obtained from the vapour tension curve of the fuel. This curve gives the minimum temperature at which the vapour has a certain pressure, and depends upon the proportions of the mixture."

The pressure p in mm. of mercury is calculated from Sorel's formula

$$p = \frac{760}{1 + V\delta}$$

where V is the volume of air in cubic metres which is mixed with 1 kg. of fuel and relates to the "proportions of the mixture" and δ is the vapour density of the fuel (on the ordinary hydrogen scale) at 15°C. and 760 mm. of mercury pressure.

THE MINIMUM TEMPERATURES IN DEGREES CENTIGRADE AT WHICH MIXTURES OF FUEL AND AIR CAN EXIST AS VAPOUR.

Quantity of Air in Mixture.	Minimum Temperature for Air and Hexane Mixture.	Minimum Temperature for Air and Heptane Mixture.
	Degrees C.	Degrees C.
Theoretical quantity	−17·7	3·6
Ditto + 20 per cent. . . .	−20·6	0·7
Ditto + 40 per cent. . . .	−24·2	−2·0

From these figures it is evident that either an excess of air must be added in order to prevent freezing and to maintain the vapour form, or heat must be added, either to the liquid or to a portion of all the incoming air.

Taking the fact that 1 kg. of petrol requires 15·6 kg. of air theoretically, to produce a perfectly carburated mixture and no heat is added during the evaporation of the liquid, the fall of temperature would amount to 18° or 19°C. according to the composition of the fuel. With a normal atmospheric temperature, this drop would result in a final temperature of $-3°$ or $-4°$C. at a gradual rate of evaporation. In the case of a motor-car engine the time taken for the passage of the air from the carburettor inlet to the engine inlet is about $\frac{1}{40}$ second, and temperatures as low as $-10°$ C. have resulted, due to evaporation in this short space of time.

It is inconceivable that the whole of the fuel can be evaporated completely during $\frac{1}{40}$ second.

A large excess of air such as the admission of 18 to 20 kg. per kg. of petrol is inadvisable, resulting as it does in the production of a large volume of exhaust gases and consequent loss of heat.

It is necessary, therefore, to allow only sufficient heat to be carried in the air to adjust the resulting temperature of the mixture so as to prevent temperatures below say 4° C. for heptane being reached as a minimum. This implies that the incoming air must not be below about 22° C. for slow running engines and 28° C. for high speed engines.

The author has for some time heated the liquid fuel in addition to slightly heating the air, but difficulties may arise through the formation of gas in the float chamber of the carburettor, especially when hill-climbing on a low gear.

The heat in this case is obtained from the exhaust, which is an erratic source of heat for the purpose.

Viscosity of Fuels under Variations of Temperature.

The utilisation of heated fuels led the author to conduct a series of experiments in order to obtain some viscosity ratios for various fuels, and to discover to what extent the addition of heat to the liquid affected its rate of flow through an orifice. When, as is usual, the head of petrol in the jet tube is regulated by means of a float, any alteration in the specific gravity of the liquid will affect the buoyancy of the float and consequently the head of petrol.

The author's figures for variation in the specific gravity due to a rise in temperature of the liquid are as follows :—

TABLE I.—FUEL TESTED: "ANGLO 0·760."

Temperature in °F.	Specific Gravity.
54	0·732
60	0·730
65	0·728
70	0·725
75	0·723
81	0·720
86	0·718
90	0·715
95	0·713
THEORETICAL.	
100	0·710
110	0·705
120	0·700
130	0·695

Curve A.

The above table roughly indicates a reduction of 0·005 in the specific gravity per 10° rise in temperature, and it will be seen that the decrease in the specific gravity is not very material, and would in itself only tend to increase the head of petrol by allowing the float to sink deeper into the liquid and retarding its action on the valve which it controls. In addition we have the effect of the lower viscosity due to a rise of temperature.

From a number of experiments which the author has conducted with different liquid fuels, the following figures demonstrate the fact that with a uniform head, the times taken for the fuel to pass through the orifice vary with the temperature as follows :—

TABLE II.—FUEL: "ANGLO 0·760" SPIRIT.

Effects of temperature upon viscosity. Head over orifice = 60 mm.

Tests of sample quantity through instrument.

—	Fuel: "Anglo 0·760" Spirit.	Petroleum Distillate between 150° and 300° C.
Temperature °F.	Time taken in seconds.	Time taken in seconds.
58	270	400
75	255	390
90	220	375
110	180	
120	165	
135	150	

The actual pressure head varied slightly owing to the decrease in the specific gravity at higher temperatures, but this affected the results only to the extent of 2 per cent. at the highest temperatures, assuming the time taken varies as the ratios of the square roots of the specific gravities.

In order to obtain comparative viscosity figures for various fuels by means of this instrument, four samples were taken and tested with heads of 30 mm. and 60 mm., and the table shows the actual times taken in each case, together with corrected values for equal pressures upon a specific gravity basis at 54° F.

TABLE III.

Fuel.	Specific Gravity.	Time Taken.				Corrected Values. Time Taken.			
		H = 30 mm.		H = 60 mm.		H = 30 mm.		H = 60 mm.	
		min.	sec.	min.	sec.	min.	sec.	min.	sec.
Pratt's spirit . .	0·715	4	0	3	10	4	0	3	10
"Anglo 0·760". .	0·730	4	0	3	15	4	3	3	18
Distillate between 150° and 300° C.	0·795	8	15	6	30	8	40	6	52
Paraffin	0·825	15	0	8	0	16	8	8	35

Thus the times taken in practice are multiplied by the square root of their specific gravity and divided by the square root of that of Pratt's spirit, which is taken as the standard in these tests.

These figures are important as showing the rate of flow of say a paraffin through a small orifice as compared with the results of tests through an actual jet, diameter 0·95 mm.

Now in the case of benzol at low pressures and through small orifices, the rate of flow being lower than that of petrol, less liquid will flow through a given aperture in unit time, the reduction in quantity is approximately proportional to the difference in thermal value of the two fuels. Thus the carburettor jet automatically adjusts the flow of benzol so as to pass quantities of equal calorific value in unit time in the case of both benzol and petrol. M. Edmond Ledoux gives the calorific values of benzol as 8844 calories * per litre, petrol as 7910 calories per litre, showing that benzol has 12 per cent. more calorific value than petrol. Also the heat required to vaporise a quantity of each liquid containing 1000 calories of heat

* In all cases "major calories," i.e. kilogramme-degree Cent.

for benzol = 12·9 calories = 1·29 per cent. } of the total heat
petrol = 14·1 " = 1·41 " " } of combustion.

From the above figures 89·3 volumes of benzol contain the same quantity of heat as 100 volumes of petrol, and from the formula

$$v = \sqrt{2\,g\,h\left(\frac{\mathrm{D}}{d}\right)}$$

where v = the velocity in feet per sec.
g = the acceleration due to gravity = 981 cm. per sec. per sec.
h = the head over the orifice in mm.
D and d are the specific gravities of the liquids under comparison

the flow of 100 volumes of petrol corresponds to that of 88·9 volumes of benzol, that is the flow through a given orifice with a constant head is proportional to the square roots of the densities.

This reasoning bears out the automatic adjustment argument in connection with the two fuels.

That the above is not strictly correct in practice is obvious from the fact that instead of there being a constant head it is the suction pressure which is constant for any given engine speed with either fuel. Also the viscosity does not vary directly as the square root of the density in different liquids. As an instance, oil and water.

In the author's opinion, borne out by results of experiments shown in Table IV. and curve H, the viscosity of benzol is almost the same as that of petrol when the greater suction pressures are applied, and only at low engine speeds and consequently smaller suction pressures is the adjustment automatic. This accounts for the fact that it is necessary to allow more air to enter the carburettor at high engine speeds when using benzol.

A study of Table IV. and curve H will show that as the pressure head of liquid increases, the difference in the relative viscosities decreases, and also the increase in the diameter of the jet tube has the same effect. Curve H shows the comparisons between benzol and petrol in this respect, and it will be noticed that with an equivalent water head of 130 mm., the rate of flow for benzol has been 4 per cent. slow with 1·40 mm. diameter jet, 11 per cent. slow with 1·20 mm. jet, 28 per cent. slow with 1·00 mm. diameter jet as compared with "0·760 Anglo" petrol under the same conditions of temperature.

TABLE IV. (A).—TIMES TAKEN FOR 2 OZ. OF LIQUID FUEL AT 55° F. TO FLOW THROUGH AN ORIFICE 0·95 MM. DIAM.

Fuel.	Specific Gravity.	Head over Orifice in mm.			Head over Orifice in mm., Corrected for Equal Pressures.		
		30	40	60	30	40	60
		sec.	sec	sec.	sec.	sec.	sec.
"Anglo 0·760" . . .	0·730	77	70	50	77	70	50
Distillate from paraffin .	0·795	165	142	..	172	148	..
Benzol	0·885	105	97	76	116	106	80

TABLE IV. (B).—TIMES TAKEN FOR 2 OZ. OF LIQUID FUEL AT 55° F. TO FLOW THROUGH AN ORIFICE 1·2 MM. DIAM.

Fuel.	Specific Gravity.	Equivalent Head for Petrol = 120 mm. Benzol = 99 mm.	Head for Petrol = 150 mm. Benzol = 124 mm.	Head for Petrol = 180 mm. Benzol = 148 mm.
		sec.	sec.	sec.
"Anglo 0·760"	0·730	62	35	30
Benzol . . .	0·885	75	37	33

TABLE IV. (C).—QUANTITIES OF BENZOL (sp. gr. 0·875) FLOWING THROUGH ORIFICES IN GALLONS PER HOUR. TEMPERATURE, 66° F.

Diameter of Orifice in mm.	Fuel Head = 120 mm. Equivalent Water Head = 105 mm.	150 131	180 157
	gal.	gal.	gal.
1·0	0·775	0·94	1·07
1·2	1·21	1·36	1·55
1·4	1·83	2·06	2·25

TABLE IV. (D).—QUANTITIES OF BENZOL (sp. gr. 0·875) FLOWING THROUGH ORIFICES IN LITRES PER HOUR. TEMPERATURE, 66° F.

Diameter of Orifice in mm.	Fuel Head = 120 mm. Equivalent Water Head = 105 mm.	150 131	180 157
	litres.	litres.	litres.
1·0	3·52	4·27	4·86
1·2	5·5	6·18	7·04
1·4	8·32	9·31	10·22

The theory that the friction of a small jet orifice is greater in the case of benzol than that of petrol is thus borne out.

Suction and Air Velocity.

Curve D is the theoretical curve showing the relation between suction and air velocity, and is based on the relation

$$v = \sqrt{2gh}$$

the formula for falling bodies, where

v = the velocity in feet per second,
g = acceleration due to gravity = 32·2 feet per sec.
h = the head of the liquid in feet.

This is drawn to two scales, the larger one reading to a velocity of 64 feet per second and showing 1 inch of water, and reading down to lower values in tenths and hundredths of an inch of water; the smaller scale reads up to 12 inches of water suction and represented by an air velocity of 220 feet per second. From this curve it is possible to find the approximate suction pressure in any part of the carburettor system for any particular engine speed, knowing of course the dimensions of the engine and the area of the aperture for which the suction pressure is calculated.

The author conducted a series of experiments with a Longuemare carburettor having an air inlet 30 mm. diameter on the engine side. The main air inlet was 25 mm. diameter = 4·9 sq. cm. area tapering towards the jet to 34 mm. diameter where it enters the jet chamber (see diagram). The suction pressure was measured in this tapered portion by means of a draft gauge, and the figures obtained are given in column 2 in Table V. In calculating the theoretical values the larger area of inlet (diameter 34 mm.) = 9 sq. cm. has been taken, and the results all fall below the actual figures obtained experimentally. The whole of these are shown in curve E and their differences in curve F.

If, however, the theoretical suctions are calculated on the smaller area, viz. 4·9 sq. cm.* a great difference is noticed, and whereas the actual figures are higher than the theoretical by an amount ranging between 0·28 inch in the lower to 0·4 inch in

* By "calculating suctions upon certain areas," the author signifies that the piston displacement in unit time has been divided by the area in question, which gives a certain linear velocity. This velocity he terms the "theoretical value of air velocity giving a theoretical suction pressure" at any point.

TABLE V.—LONGUEMARE CARBURETTOR.

Engine revs. per min.	Suction at Inlet (area 9 sq. cm.) in Inches of Water.		Velocity of Air at Inlet to Engine side of Carburettor. Taken from Curve.		Velocity of Air surrounding the Jet. Extra Air Inlets closed. Total Area of Passage = 1·74 sq. cm.		Suction in Inches of Water on Area of 1·74 sq. cm. *(calculated).	Velocity of Air with extra Air Inlets open. Total Area of Inlets = 5·4 sq. cm.		Suction in Inches of Water on Area of 5·4 sq. cm. *(calculated).	Suction in Inches of Water, at Main Air Inlet Area = 4·9 sq. cm. *(calculated).	Velocity in Feet per sec. Corresponding to Suctions in previous column.
	Experimental.	Calculated.	Metres per min.	Feet per sec.	Metres per min.	Feet per sec.		Metres per min.	Feet per sec.			
360	0·5	0·22	560	30	2955	161	6·5	932	51	0·65	0·78	56
480	0·75	0·41	746	41	3942	215	11	1242	67·7	1·1	1·3	75
600	1·0	0·64	933	51	4930	..	..	1552	85	1·6	2·0	94
640	1·1	0·75	995	54·3	..	..	..	1650	90	1·8	2·2	100
680	1·25	0·82	1057	58	..	.	..	1760	96	2·0	2·5	106
820	1·5	1·1	1274	70	..	..	..	2122	116	3·2	3·8	128
1000	2·0	1·6	1552	85	..	.	..	2590	141	5·0	6·0	156
1250	3·0	2·6	1940	106	..	..	..	3235	176	7·7	9·0	195
Column 1	2	3	4	5	6	7	8	9	10	11	12	13

* For explanation see footnote, page 8.

the higher values, yet when the smaller area of inlet is calculated upon, the differences are in the opposite direction, i.e. the experimental results are below the theoretical ones by amounts varying between 0·28 inch in the lower to 6 inches in the higher values.

The diagram shows the dimensions of the different portions of the carburettor tested, and the author leaves for discussion the question as to which area the theoretical suction should be calculated upon. In the lower values the actual suction is about the mean of the two theoretical suctions.

Table V. shows the air velocities together with their corresponding suctions when the smallest choke tube (30 mm. diameter) is used and when the extra air inlets are open or closed. It will be seen that when the engine speed rises above 500 revs. per minute an abnormal suction occurs until the extra air inlets are open. The effect of opening these in this case is to reduce the suction to one-tenth its previous amount.

The Passage of Petrol through a Single Orifice.

The object of the experiments contained in this series was to obtain some practical data for the use of the motoring world at large, as distinct from purely theoretical deductions.

The test apparatus consisted of a small brass tank having a tube fixed into the bottom which terminated at the other end in a tee piece. Into this tee piece jet tubes were screwed in turn, each jet tube having been carefully drilled to $\frac{1}{100}$ mm. in diameter. The tubes used in the experiments ranged from 0·95 mm. to 1·40 mm. diameter in the bore, and the head of the liquid was varied within wide limits.

For the first series pressure heads between 30 mm. and 90 mm. were taken in order to ascertain the probable amount of friction in passing through the tube at low speeds, but with the 30 mm. heads (= 22 mm. of water) less liquid passed through than would be the case in actual practice except under conditions of no load or very light load. The 90 mm. head (= 66 mm. of water) corresponds to the suction when the car is running on the level with throttle partially open, the area of the aperture being then 2·5 sq. cm., full throttle area being 3·8 sq. cm.

In carrying out these experiments the fuel in the tank was kept at a constant level and the time noted in which a given quantity of fuel passed through the orifice in the jet tube. This was done for heads of 30, 60, and 90 mm. and upwards with each size of jet, and the appended Table VI. shows the quantity of fuel passing through the orifice in gallons per hour in addition

to the time taken in seconds for the sample quantity. Intermediate values have been filled in from the curves produced experimentally, and the values have been cross checked, assuming that the flow has been proportional to the area of the jet or to d^2 and to the square root of the head $= h$.

TABLE VI.—CLAUDEL HOBSON CARBURETTOR.

Diameter of Orifice in mm.	Fuel Head in mm.	Quantity Flowing in Gallons per Hour = $Q = \frac{45}{t}$.	Time taken for Unit Quantity to Flow through = $t = \frac{3600}{80 \times Q}$.	Diameter of Orifice in mm.	Fuel Head in mm.	Quantity Flowing in Gallons per Hour = $Q = \frac{45}{t}$.	Time taken for Unit Quantity to Flow through = $t = \frac{3600}{80 \times Q}$.
			secs.				secs.
0·95	30	0·32	140	1·20	30	0·515	87
„	60	0·454	99	„	60	0·725	62
„	90	0·562	80	„	90	0·895	50
„	120	0·645	69	„	120	1·03	43
„	150	0·725	62	„	150	1·16	38
1·00	30	0·352	127	1·25	30	0·56	80
„	60	0·51	88	„	60	0·786	57
„	90	0·62	72	„	90	0·97	46
„	120	0·715	62	„	120	1·116	40
„	150	0·805	55	„	150	1·25	35
1·05	30	0·392	114	1·30	30	0·608	73
„	60	0·554	81	„	60	0·85	52
„	90	0·684	65	„	90	1·052	42
„	120	0·786	57	„	120	1·208	37
„	150	0·886	50	„	150	1·36	33
1·10	30	0·433	104	1·35	30	0·655	68
„	60	0·61	73	„	60	0·915	49
„	90	0·752	59	„	90	1·13	39
„	120	0·865	52	„	120	1·30	34
„	150	0·974	46	„	150	1·465	30
1·15	30	0·474	95	1·40	30	0·705	63
„	60	0·665	67	„	60	0·987	45
„	90	0·821	54	„	90	1·216	37
„	120	0·943	47	„	120	1·4	32
„	150	1·064	42	„	150	1·58	28

Certain experimental errors have crept in, particularly at the lower values, owing to the orifice at times becoming partially fouled, but in the table these errors are neglected and approximately true values given. On the whole, however, the experimental points have agreed very well, and the curves have been located upon the majority of the points obtained.

In conducting these tests it was remarkable how easily the flow through the smaller orifices became erratic, which may account for the difficulty which is often experienced in practice

in running an engine very slowly for any length of time. In many cases constant pricking of the orifice became necessary in order to obtain any sort of consistency in the data. The curves obtained are shown in curve C for a fuel temperature of 55° F.

Now it was noticed that an increase in the temperature affected the viscosity as shown in Table II. when the fuel was tested with the instrument, therefore samples were tested at 70° F. in the jet apparatus, and the results compared with those obtained at 55° F. are shown plotted in curve K. These upper curves are again shown in curve H compared with experimental values for the flow of benzol at the same temperature, with the pressure heads reduced to a water standard in each case.

It will be noticed, for instance, that with a jet diameter of 1·40 mm. the rate of flow with a pressure head of 180 mm. is 1·8 gallons per hour at 55° F. and that it increases to 2·14 gallons per hour when the temperature of the liquid is increased to 70° F. Again, with a jet diameter of 1·20 mm. and a head of 150 mm. the flow at 55° F. is 1·2 gallons per hour and at 70° F. increases to 1·5 gallons per hour.

The series of jets upon which these tests were made were from the Claudel-Hobson carburettor. The author fitted one of these carburettors to his engine so that the results could be verified in the reverse direction. Whereas the bench tests consisted in pressure head tests, those on the road were suction tests.

A large number of these were carried out with (*a*) open throttle, (*b*) partial throttle, (*c*) on top gear, (*d*) on second gear, (*e*) on first gear.

The actual suctions at various engine speeds were obtained by means of a water manometer, the tube from which was taken to a position in the main air inlet, and afterwards a small diameter tube was entered below the air strangler chamber. Some of the results of these experiments are given in Table VII. and curve G, and taken in conjunction with curve C it becomes an easy matter to find the rate of flow of petrol through the orifice under any given conditions.

Explanation of Curve G and Table VII.

A study of curve G will show the effect of having the throttle full open or partially closed. With a full opening the suction increases along practically a straight line, slightly falling off at maximum rates of revolution of the engine. This effect is more marked in the case of suctions observed at the throttle itself.

TABLE VII. (A).—EXPERIMENTAL SUCTIONS IN INCHES OF WATER AT CARBURETTOR INLET.

Miles per Hour.	Engine revs. per min.	Running Light.	Running on Level on Low Gear.	Full open Throttle. Top Gear.	
				On Hills.	On Level.
		in.	in.	in.	in.
16	500	..	..	2·2	..
18	560	..	..	3·0	0·75
20	630	..	..	4·0	..
22	700	1·4	..	..	2·3
24	750	..	2·2	5·5	..
25	790	..	..	6·0	..
25·5	800	1·6	2·5	..	..
28	880	..	..	7·0	..
28·6	900	..	3·6	..	4·4
30	940	..	..	7·8	..
32	1000	2·3	7·0	..	..
33·3	1050	3·0	..	8·8	6·0
37	1150	5·0	..	9·5	..

TABLE VII. (B).—EXPERIMENTAL SUCTIONS AT THROTTLE IN INCHES OF WATER.

Miles per Hour.	Engine revs. per min.	Running Light on Second Gear.	Throttle Full Open on Top Gear.
		in.	in.
19	600	1·9	1·9
25·5	800	2·4	2·4
28	880	2·8	..
30	940	3·2	3·0
33	1040	4·0	3·5
35	1100	4·5	..
38·3	1200	6·0	4·5
42	1320	..	4·5

On the contrary, when the engine runs fast with the throttle closed down, the curve takes an upward turn at the highest engine speeds. This is most marked when the engine runs absolutely unloaded, and with the car running on the level with the low gear in operation.

This increase of local suction is due to the small aperture through the throttle, and is more marked as the aperture is reduced in size.

The observations below the strangler are probably out of proportion to those at the main inlet, as the observation hole was very small and it was necessary to introduce a fine glass tube in place of the larger rubber tube in the first set of observations. The small tube observations, however, bear similar relations to

each other under the conditions of throttle opening, as those taken with the larger tube. A theoretical curve "calculated upon an area of 3·8 sq. cm." is shown for comparison, the theoretical air velocity is obtained relating to this area, and the suction pressure value taken from the air velocity curve D, and it will be seen how very nearly the experimental values agree with these.

Table VIII. and curve H are theoretical ones calculated directly from the measurements of the particular engine and carburettor tried, and show the velocity of air at the carburettor inlet, which was 9·2 sq. cm. in area, the area of inlet to the air strangler being 250 sq. mm., and the hole in the jet 1·05 mm. diameter.

The suctions have been calculated from curve D, given in the paper. As in the case of the Longuemare carburettor, the air openings had to be increased at about 500 revs. per min. of the engine as the suction exceeded practical limits for the jet.

The last column in the Table gives the calculated values on a full opening of the throttle of 3·85 sq. cm. in area. Now the values obtained in this manner from curve D will be minimum values as the throttle is not always full open, so in order to obtain true values the maximum must be found, and this is calculated from curve C and Table VI. giving the flow through jet tubes.

The mean of these two values will be obtained in actual practice, and in order to verify this reckoning the author carried out experiments with three different fuels, reducing the suction in each case to water standard.

The results of the tests are given in Table IX. The values obtained from actual experiment, column 7, are practically the means of columns 8 and 9. Experiments of this nature are exceedingly difficult to carry out with any great degree of accuracy, owing to the variations of road conditions.

The object of this series of investigations was to show how it becomes an easy matter to ascertain the size of carburettor orifice to satisfy prevailing conditions of cylinder and carburettor dimensions, and to point out to the motorist some of the interesting problems in connection with the carburettor. Although laboratory accuracy has not been attempted, the results obtained have been sufficiently consistent and interesting to be embodied in the form of a paper.

The author is unaware of any paper on this subject having been previously read in this country, and he will have achieved his object (if this matter is clearly understood) in showing that by simple reasoning certain important but small dimensions in connection with carburettor orifices can be arrived at.

TABLE VIII.—CLAUDEL HOBSON CARBURETTOR.

Miles per Hour.	Engine revs. per Min. = N.	Velocity of Air at Carburettor Inlet.		Suction from Curve.		Strangler Chamber; half throttle. Area = 2·5 sq. cm.				Strangler Chamber; throttle full open. Area = 3·8 sq. cm.			
						Air Velocity.		Suction (Theoretical).		Air Velocity.		Suction (Theoretical).	
		Metres per Min. =1·52×N.	Feet per Second.	In mm. of Water.	In Inches of Water.	Metres per Min. = 5·6 × N.	Feet per Second.	In mm. of Water.	In Inches of Water.	Metres per Min. = 3·d8 × N.	Feet per Second	In mm. of Water.	In Inches of Water.
11·5	360	548	29·8	5	0·21	2016	110	70	2·8	1325	72	30	1·2
13	415	632	34·5	7	0·29	2320	126	111	4·4	1528	83	38	1·5
15	470	715	39	9	0·37	2630	144	127	5·0	1730	94	48	1·9
19·2	600	914	49·9	15	0·61	3360	183	205	8·1	2205	120	86	3·4
25	780	1187	64·7	25	1·01	4360	238	..	..	2865	156	150	6·0
27	845	1286	70·4	30	1·2	4730	258	..	..	3105	160	180	7·1
32	1000	1520	83	38	1·5	5690	306	..	..	3680	206	260	10·2
35	1094	1605	91	45	1·8	6125	334	..	..	4050	220	290	11·5
40	1250	1900	103·6	61	2·4	7000	382	..	..	4600	251	..	..
Column 1	2	3	4	5	6	7	8	9	10	11	12	13	14

TABLE IX.—CLAUDEL HOBSON CARBURETTOR TESTS FOR FLOW THROUGH ORIFICE 1·05 MM. DIAM.

Fuel used, "Anglo 0·760" = 75 per cent. Benzol sp. gr. 0·88 = 25 per cent. Sp. gr. of mixture = 0·750. Temp. = 70° F.

Duration of Test in Minutes.	Miles Covered.	Speed in Miles per Hour.	Fuel used in Gallons.	Miles per Gallon.	Gallons per Hour.	(Experimental) Suction in mm. of Water.	Equivalent Water Head over Orifice producing flows of Fuel in Column 6 (obtained from Curve C.), Corrected for temperature.	*Calculated Suction on Area of Throttle Assumed Full Open (obtained from Curve D.)	Volumes of Fuel admitted per 100,000 volumes of Air at Cylinder Temperature and Pressure.
							mm.	mm.	
10	2·125	12·75	0·125	17	0·75	57	90	31	10·3
8	2·25	16·86	0·125	18	0·937	76	120	40	9·6
11	2·38	12·96	0·125	19	0·795	57	96	31	10·5
14	4·00	17·1	0·187	21·4	0·802	76	97·5	40	8·1
Fuel "Shell Spirit" sp. gr. = 0·715.									
6	2·38	23·8	0·125	19	1·25	140	179	114	9·0
5	2·25	27·0	0·125	18	1·5	178	236	137	9·5
Fuel "Anglo 0·760" sp. gr. = 0·730.									
5·5	2·38	26	0·125	19	1·36	153	212	126	9·05 †
7	2·38	20·4	0·125	19	1·07	110	139	72	9·0
Column 1	2	3	4	5	6	7	8	9	10

* For explanation see footnote, page 8.

† 26 miles per hour = 816 revs. per min. Volume swept per rev. = 1400 c.cm. per hour = 68·5 million c.cm. Volume of fuel per hour = 6180 c.cm.; and the ratio of volume of fuel to volume swept out = 9·05 to 100,000.

The author hopes that by these methods, rule-of-thumb trials can be, to a certain extent, eliminated, and a clearer notion of the action of a carburettor orifice obtained by the average motorist.

The author wishes to thank the staff in his office who have assisted in working out results and prepared the curves and drawings.

APPENDIX.

Since the author read his previous paper before the Society of Engineers, a large firm of distillers of benzol, the Semet-Solvay Co., of New York, have carried out a series of tests on the lines of those described in the paper. The engineer of the company, Mr. E. A. Barnes, has now furnished the author with the results of these tests, which agree very favourably with the results already published. In comparing the distillation figures it must be noted that the percentages refer to the distillation at 100° C. in the American tests, whereas those in England are reckoned at 120° C., that is to say 90 per cent. benzol implies the boiling off of 90 per cent. of the volume at or under 100° C. In conversation with the author, Mr. Barnes pointed out that in spite of America being a petroleum producing country, benzol was making considerable progress as a fuel for internal combustion engines.

Experiments on the Use of Benzol in Petrol Engines.

Brake test on a Cadillac 4-cylinder 20-H.P. engine, cylinders 4-inch bore, 4½-inch stroke, water cooled, normal compression 75 lb. per sq. in.

The carburettor was arranged so that air could be taken in both at the main and auxiliary openings from a sheet iron jacket surrounding the exhaust. The air thus secured was so hot as to make the inlet-pipe unbearable to the touch.

A Prony brake was used, the brake-wheel being bolted direct to the engine shaft.

Petrol.—The engine was first tested on petrol, the hot-air pipe being removed.

At 1000 R.P.M. and 18 H.P., the thermal efficiency was 18·87 per cent. This is good laboratory practice for this type of engine.

Benzol.—Lebanon light oil No. 303, distilling about 83 per

cent. at 100° C. This oil was unwashed and had the usual disagreeable odour, but was entirely free from naphthalene.

The hot-air pipe was put on and during test was warm to touch. No carburettor adjustments were made. The engine started easily and ran perfectly, the exhaust being clear and odourless. After test the sparking plugs showed no sooting.

Two 20-minute runs were made at 1000 R.P.M. and 16 H.P. The average thermal efficiency was 19·8 per cent.

Benzol.—Lebanon light oil No. 302, showed same distillation test as No. 303. The colour, however, was darker than that of No. 303, being almost black against a dark background, and dark green when transmitting light.

The oil had in suspension black particles of foreign matter, like carbon, and a small quantity of globules, like heavier oil.

The engine started, ran for a few minutes, stopped, and could not be started again.

The carburettor needle-valve was clogged with heavy viscous matter.

After waiting a day the engine ran perfectly on the same oil, the difference probably being due to the oil being shaken up at first. After lying a day the foreign matter would settle down, and the oil would be practically filtered.

Two 20-minute runs were made at 1000 R.P.M. and 16·5 H.P. The average thermal efficiency was 18·7 per cent.

The exhaust was clear and odourless, and the sparking plugs were free from soot. The hot-air pipe was used.

A short capacity test gave 21·6 H.P. at 1200 R.P.M. This test was not conclusive.

Benzol.—Wheeling light oil, distilling 78 per cent. at 100° C. and 97 per cent. at 130° C.

With the throttle closed the engine ran fairly regularly, but when opened the engine slowed down and stopped.

The failure of this test may have been due to the fact that the hot-air pipe was not on the auxiliary air-supply.

After an interval of three weeks the same oil was tried mixed with 10 per cent. denatured alcohol. The hot-air pipe was on.

The engine started and ran well. Exhaust was clear and sparking plugs were clean.

The thermal efficiency with a load of 17·4 H.P. at 1000 R.P.M. was 19·7 per cent.

At the end of 20-minutes test the load was reduced to 6 H.P. and the engine ran for one hour, the carburettor then showed only slight traces of a residue of heavy oil.

Another test was made with the wheeling oil only and the hot-air pipe on.

The engine gave slight trouble in starting, but otherwise ran well, and a 20-minutes test was made, the thermal efficiency being 13·6 per cent. The carburettor showed slight traces of heavy oil.

Benzol.—Syracuse light oil, containing about 50 grammes per litre of naphthalene. The engine started well, but after a short time the carburettor became clogged with naphthalene. A trial was then made with 50 per cent. Syracuse and 50 per cent. alcohol. The engine ran well light, but would not carry any load. The carburettor was again found full of naphthalene.

The following percentage mixtures were also tried:—

Alcohol	75	50	25	20	15	10
Benzol Wheeling L.O.	25	50	75	80	85	90
	100	100	100	100	100	100

	per cent.		per cent.
Alcohol	50	Alcohol	50
Petrol	50	Benzol Lebanon L.O.	50
	100		100
Alcohol	$33\frac{1}{3}$	Alcohol	50
Benzol Wheeling L.O.	$33\frac{1}{3}$	Benzol Wheeling L.O.	25
Petrol	$33\frac{1}{3}$	Petrol	25
	100		100

With all the benzol mixtures the operation was very regular, and it was possible to carry a good load.

TABLE OF RESULTS.

—	Petrol.	Lebanon Benzol, No. 302.	Alcohol.	90 per cent. Benzol, 10 per cent. Alcohol = 100 per cent.
Cals. per kilo	11235	9979	6572	9775
Cals. per litre	7808	8712	5362	8354
Specific gravity	0·695	0·873	0·82	0·856
Horse-power	18·1	16·6	17·5	17·4
R.P.M.	1000	1000	1000	1000
Lb. per B.H.P. hour	0·67	0·749	1·16	0·73
Galls. „ „	0·115	0·1035	0·116	0·100
Thermal efficiency per cent.	18·87	18·7	18·5	19·7

The mixtures containing 50 per cent. and 75 per cent. alcohol gave trouble in starting, the engine having first to be warmed on petrol. The others started easily.

As the alcohol increased, the auxiliary air valve had to be shut down more and more until it was tight shut. On all other mixtures the engine started and ran perfectly, carrying a good load.

Comparison between petrol and benzol on a motor truck. Engine 2-cycle, 4-cylinder, developing 40 H.P. The tests were made over the same road in each case.

—	Petrol.	Benzol.
Gross load in lb.	10535	10380
Fuel used in galls.	4·15	3·20
Fuel used per gross ton in gall.	0·787	0·616
Relative economy	1·0	0·782

Increased economy of benzol over petrol = 21·8 per cent.

Comparison between petrol and benzol on a 6-cylinder, 35 H.P. touring car. The route in each case consisted of 2 laps of $8\frac{1}{2}$ miles, the road was dry but rough in parts, necessitating several reductions of speed.

—	Petrol.	Benzol.
Time for covering the course	41 min. 16 sec.	36 min. 7 sec.
Length of course, miles	17	17
Average speed, miles per hour	24·7	28·2
Time for best lap	19 min. 51 sec.	17 min. 11 sec.
Average speed, best lap, miles per hour	25·7	29·7
Fuel consumed, gallon	1·15	1·019
Fuel consumed per mile, gallon	0·0676	0·0600
Fuel consumed per mile, ratio	1·13	1·0
Miles per gallon of fuel	14·78	16·68
Miles per gallon of fuel, ratio	1·0	1·13

Comparison between petrol and benzol on a motor boat. Engine 2-cylinder, 2-cycle, speed = 750 R.P.M., H.P. = 10, giving boat speed of 12 miles per hour. A course of 8 miles was run in each case.

The operation on benzol was very satisfactory. Combustion was more nearly perfect, giving a clear exhaust instead of a smoky exhaust as with petrol.

—	Petrol.	Benzol.
Time for covering two miles	9½ min.	10⅓ min.
Time for covering course	38 min.	39 min.
Fuel used, c.cm.	2700	2400
Fuel used, gallon	0·713	0·634
Fuel used, ratio	1·12	1

Conclusions.

These tests indicate that unwashed benzol free from naphthalene with the distillation test in the neighbourhood of 80 per cent. at 100° C. can be used with satisfactory results in the internal combustion engine.

Economy over petrol ranges from 10 per cent. to 13 per cent. by volume.

A slight difference in carburettor adjustment is desirable, but not essential.

With ordinary care there is no sooting of sparking plugs, excessive deposits of carbon, or sticking of the valves.

The disagreeable odour of the fuel in the liquid state is an objectionable feature. The exhaust, however, is odourless when the proper adjustments are adhered to.

DISCUSSION

The President said that those present would remember the valuable paper which the author read to the Society thirteen months ago, and the excellent discussion they had upon it. They now had to thank him for coming forward again and giving them the results of his further experiments and investigations. It was clear that some more reliable data were required on the matter, and notice should be taken of what the author said in the paper that 'The object of the experiments contained in this series was to obtain some practical data for the use of the motoring world at large, as distinct from purely theoretical deductions." They had all met with papers which were obviously written not so much to elucidate special points in scientific research as to show that the authors were clever men, and could indeed produce papers which hardly anybody else could understand. In the present case it was just the opposite; and he proposed that the thanks of the Society be given to the author for his paper.

The proposal was carried by acclamation.

Mr. Worby Beaumont said that the author had made experiments and had taken a great deal of trouble to give them the results. While crediting the author with every desire to help those who tried to arrive by calculation at results, which, at present they could only reach by experiments, he thought that the author had failed to help them much. Italy, France, Belgium, Holland, Germany and Great Britain, were all struggling for pre-eminence in the construction of motor vehicles using petrol and the like as fuel, and they all had to use carburettors. Many experiments and calculations had been made with a view to finding out how a carburettor should be designed, yet notwithstanding the really scientific work which had been done, no one in those several countries had made a carburettor which would perform all its functions with certainty. That was because nobody had yet considered the whole of the controlling influences affecting a carburettor. One of the most effective carburettors which he knew of was a thing which cost about 3*d.*, in the shape of a little squeeze oil-can. When no other carburettor would cause an engine to work, or at all events to start, it would squirt a little petrol into the right place, and it found its own quantity of air. Provided that they got the petrol into the thing that had to burn it, and somewhere near the thing that was to set light to it, and in a sufficiently divided form with some air, they obtained combustion. Many people had made carburettors with very pretty arrangements of jets impinging in this or that direction, but they had forgotten that the petrol was not sent in under pressure and that it only passed through the jet because there was an inducing current of air which would, if it had the opportunity, take with it a certain amount of petrol. They had forgotten, too, that if the passages through which that air and the petrol went were not absolutely smooth and straight, the petrol would get broken up with the air in its course towards the cylinder of the engine, and that the air went in very much like a rope of air pulled into the tube, carrying with it, in its own direction, and only in that direction, whatever happened to be injected. When the passage would allow a suitable motion and velocity of the air, and when the petrol was broken up into a very fine spray, then, as a rule, it was found that they got the best results, and they found that it did not matter whether a certain part of the petrol, in the early stages of its movement as between the carburettor and the inlet valve of the engine, was more rapidly vaporised than the rest, or not. What was wanted was a petrol fog, but it was found that the obtainment of that depended upon a great many things.

The author had made experiments upon the rate of flow, more particularly through the jets of two kinds of carburettors.

He (the speaker) was sorry to say that the larger proportion of carburettors in use were not these types, but were such as presumably the author had used for his experiments on the flow under a steady head of petrol with the apparatus shown on the diagram. The rates of flow under a head as steady as the author could maintain it by gradually filling the petrol into the supply tank, had an academic interest, but they were of no interest to the man who had to make a carburettor. The difference between the rate of flow through a jet under a steady head of petrol and the rate of flow through the same jet under the pulsating influence of the piston of a petrol engine, was so great that what happened in the one case was no indication of what would happen in the other. With regard to what the author had said about the "rule of thumb" he (Mr. Beaumont) supposed that he must be charitable enough to take that merely as a *façon de parler*. He objected, on the part of those who had done all that had been done, up to the present time, to the use of the phrase "rule of thumb" in its ordinary acceptation. He did not suppose that any prettier mathematical investigation had ever been made than that into the question of the rate of flow of air and petrol by several of the well-known men, who, more particularly in France and Belgium, had produced the carburettors used at the present time.

The author had given a number of calculations and results with regard to air velocities, which would no doubt be of service to many who might have to deal with the matter, but it must be remembered that the way in which air flowed through a hole or a pipe differed very largely according to whether it was air sent into the atmosphere or into a vacuum or partial vacuum, and that it varied materially in proportion as the vacuum into which it might be passing increased or decreased in its quality as a vacuum. That point had to be considered when carburettors were being dealt with or when anybody spoke of doing by calculation that which is generally only solvable by experiment. The rate of flow when calculated with reference to the usual expression $\sqrt{2gh}$, even with the qualification given in the paper, did not enable one to make a carburettor without having to do a good deal of experimenting to get it, quite right for any particular engine.

The figures given were all of interest, but he should like to warn the author that he aspired too much if he wished it to be supposed that those who had done what had been done in the production of the best carburettors were helped from a condition of rule of thumb to one of theoretical accuracy in building up a carburettor for a given purpose. The author had made some remarks which were not in the paper. One remark was that for

carburettors certain experiments "agreed very fairly," and other phrases were "in all probability," and "they are within 10 per cent. of"—this and that. That was exactly what other people had found. They had found that for a carburettor they had to use the words, "in all probability," and while he repeated his thanks to the author, he would like to ask him to go a little further than he had gone, and to take into consideration the very many small things which affected not only the flow of petrol, but the flow of air intermittently taken through holes, slots, ports, and valves into the cylinder or cylinders of a petrol engine.

Professor R. H. Smith said that the experiments that were most interesting to him were those on the decrease of viscosity and of specific gravity with a rise of temperature. The results shown in the paper were not only of scientific interest, but also of practical importance. With regard to the two curves, A and B, the author had not pointed out that they were quite similar, because although curve A appeared as a straight line, still throughout the temperature range of A, namely, up to 95° F., curve B was also practically a straight line. Curvature in A would probably appear if the diagram were extended to higher temperatures. With regard to the fundamental equation which the author had relied upon a good deal in his calculations, namely, that the velocity of air in an air jet was proportional to the square root of the head, he had taken account of the variation of viscous resistance with temperature, but not its variation with the velocity itself. That variation was very great, and though the propelling force was proportional to the gradient of pressure, the propelling force was not the only thing which influenced the result. Though the discovery of the scientific law of the dependence of the flow of air, or air laden with petrol fog, for a steady head was of fundamental importance, and was the base upon which other results must be built up, still they did not have a steady head in petrol and gas engines during admission of the charge. There was a very rapid pulsation of driving head, and the result of that was that a great deal of that head was spent in acceleration of momentum which, he thought, was not taken into account in the author's calculations. The speed at which fluids and oil vapour passed into the cylinder, and through the passages on the way to the cylinder, varied through large ranges in very short times, so that the rate of change of velocity was enormous. That meant great accelerations of momentum.

Mr. J. Veitch Wilson said that he was rather interested in the author's statement that benzol required a very different percentage of air from petrol, and he was interested also to find

him mention a thing which had often occurred to himself in his battles with motorists, that petrol was not really volatilised, but was atomised. It had been his misfortune to have had sent to him motor valves, induction pipes and other things, to prove that the material which he was responsible for supplying, namely, the lubricant, was misbehaving itself, and instead of lubricating, was depositing itself on the valves with disastrous consequences. He had observed that the deposits occurred on the outer side of the valves and on the induction pipes, and he formed the idea, with the assistance of his firm's chemists, that the deposits were not really oil deposits, but were actually atoms of unvolatilised petrol which had impinged on the bend of the pipe or the outer side of the valve, and by condensing and slowly indurating there, had turned into pitch.

All interested in petrol motors were well aware that 0·680 or 0·700 sp. gr. spirit was by no means necessarily 0·680 or 0·700 spirit. It might be a mixture of different fractions adjusted to give what was supposed to be the proper specific gravity. Many years ago, in order to get at that, a number of synthetical mixtures had been made, which are as follows:—

Pratt's motor spirit	Percentages	100	..	..	..	..	..
„ gasolene		..	100	..	..	35	73
„ benzoline		..	..	100	..	65	..
White rose petroleum		..	..	..	100	..	27
Sp. gr. of material tested		0·680	0·640	0·700	0·786	0·680	0·680
		°F.	°F.	°F.	°F.	°F.	°F.
First drop		97	74	117	288	80	76
5 per cent.		119	88	152	325	111	90
20 „		132	97	167	355	132	104
50 „		156	112	189	412	165	136
90 „		220	165	248	532	240	459
95 „		282	207	282	576	295	599

He presented a table (p. 222) which showed how very much petrol might be broken up in use. Perhaps he might quote some of those figures. They were curious. With Pratt's motor spirit they found the first drop at 99° F. and got 99 per cent. distillate at 275° F. They obtained also a small quantity of partly used spirit which gave the first drop at 147° F. and 99 per cent. at 390° F., a very much higher point, showing that the used spirit had not been evaporated uniformly. Spirit which had been some time in stock gave first drop at 109° F. and did not give 99 per cent. until 380° F., showing the effect of age upon spirit, which, he thought, would no doubt tell against it in use.

He had made a number of tests with heavy oils. Those tests would be of interest to those who were connected with motoring,

—	Specific Gravity.	Distillate.					
		First Drop.	5 Per Cent.	20 Per Cent.	50 Per Cent.	90 Per Cent.	99 Per Cent.
		At °F.	At °F.	At °F.	At °F.	At °F.	At °F.
Pratt's motor car spirit .	0·648	99	116	133	156	206	275
Ditto, ditto (partly used) .	0·710	147	163	177	196	268	390
Ditto, gasoline	0·647	80	95	106	120	158	203
Anglo Am. Co.'s 0·700 petroleum spirit . . .	0·710	109	124	150	192	290	380
Ditto, ditto (old sample) .	0·691	106	135	162	196	305	..

if he could get them to realise the enormous effect of increasing temperature in reducing the viscosity of an oil. It was no use saying that a heavy oil took 2400 seconds to vaporise at 70° and came down to 74 seconds at 350° F., but he had had the figures reduced to a simple percentage basis.

Oils.	Specific Gravity at 60° F.	Viscosity at 70° F.		Viscosity at 212° F.		Viscosity at 350° F.	
		Actual.	Per Cent.	Actual.	Per Cent.	Actual.	'Per Cent.
Sperm	0·878	323	100	88	27·2	68	21·1
Gas engine.	0·907	845	100	89	10·5	68	8·1
Heavy gas engine. . .	0·905	2430	100	110	4·5	74	3·0
Motor oil (water-cooled) .	0·891	5003	100	178	3·6	88	1·8
„ (medium) . .	0·894	6655	100	181	2·7	94	1·4
„ (air-cooled) . .	0·893	9200	100	220	2·4	96	1·0

In this Table the viscosity of each oil at 70° F. is taken as unity.

Sperm-oil, which was an oil of low viscosity, retained at 350° F. 21 per cent. of its original viscosity, which was a very high percentage. A gas-engine oil gave 8 per cent. and a heavy gas engine oil 3 per cent. at the same temperature, whereas ordinary motor oils came down to 1·8, 1·4, and 1·0.

To permit of the comparison of one oil with another the viscosity of the same oils are reproduced in the Table (p. 223) on the basis of sperm oil at 70° F. as unity.

It followed from that, that a person who wanted to obtain a good body at a high temperature must make his lubricating arrangements in such a way as to permit of the use of a very thick oil at normal temperatures. As regards viscosity, he did not know what viscometer Mr. Brewer had used, but very much

would depend upon the apparatus. The instruments with which he (the speaker) had been acquainted, which were for heavy oils, would not lend themselves to dealing with such light liquids as spirit. There were three or four, including Poiseuilli's (referred to by Osborne Reynolds), Archbutt's and others, which might be adapted for the measurement of what was called absolute viscosity.

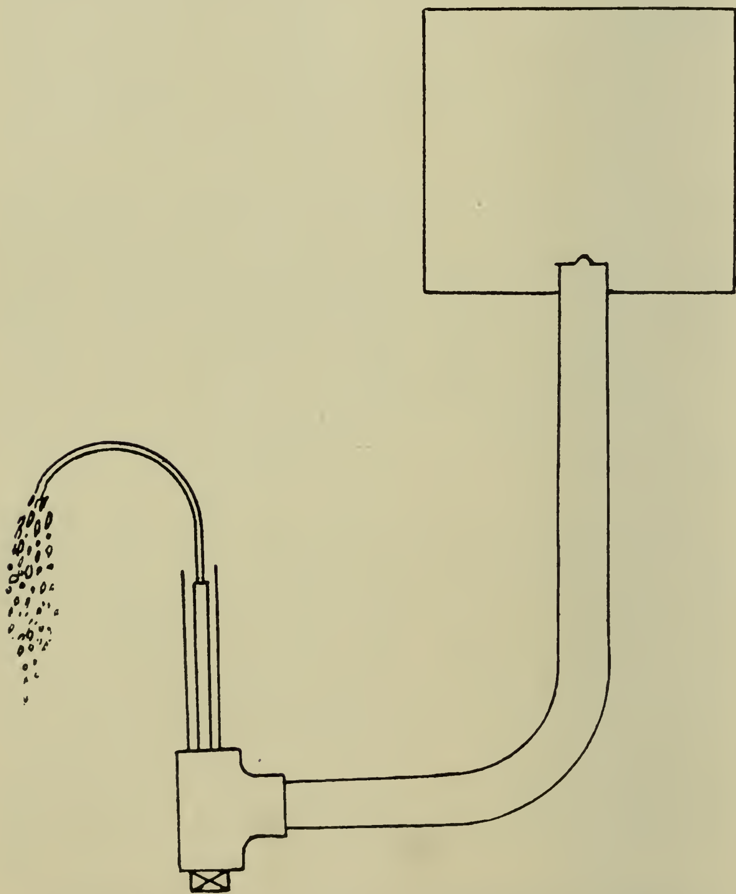
Oils.	Relative Viscosity of various Oils. Sperm at 70° = 100.					
	Viscosity at 70° F.		Viscosity at 212° F.		Viscosity at 350° F.	
	Actual.	Sperm at 70° = 100.	Actual.	Sperm at 70° = 100.	Actual.	Sperm at 70° = 100.
Sperm	323	100·0	88	27·2	68	21·1
Gas engine	845	261·6	89	27·6	68	21·1
Heavy gas engine . .	2430	752·3	110	34·1	74	22·9
Motor oil (water-cooled)	5003	1548·9	178	55·1	88	27·2
" (medium) . .	6655	2060·4	181	56·0	94	29·1
" (air-cooled) .	9200	2848·3	220	68·1	96	29·7

In this Table all viscosities are given in relation to that of Sperm Oil at 70° F. which is taken as unity.

Mr. Douglas Leechman asked the author whether he had made any experiments with nozzles having other than a smooth bore. One or two carburettors had been constructed with either screw threads, or with notches formed in the interior, with the object of setting up eddies, which would become more powerful as the rate of flow through the nozzle tended to increase, and in that way to obtain either a constant flow through the nozzle, or a rate of flow that would increase approximately with the rate of the inflow of the air. He believed that carburettor designers had found that a good deal of trouble arose from the fact that at the higher speeds, or when the suction was stronger, they got more petrol in proportion to the air that was taken in. The object of tapping or otherwise serrating the interior of the nozzle was to prevent that, and to keep the flow of petrol in proper proportion to the flow of air.

He hoped that the author would not feel too much disconcerted by Mr. Worby Beaumont's remarks. He was sure that Mr. Beaumont's endeavours were exactly on the same lines as those of the author, namely, to advance knowledge on the subject. They must all feel that Mr. Brewer had at any rate done very useful work, and he did not suppose that Mr. Brewer would tell them that there was nothing else to be done.

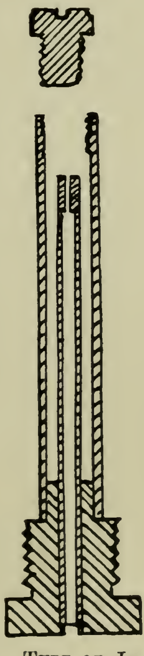
Mr. J. Lyons Sampson said that in the first paragraph of the paper the author spoke of the condition of the petrol in its passage from the jet to the cylinder, as to whether it went in the form of spray. He (the speaker) thought that there could be no doubt that a great deal of the petrol which went into the



Arrangement of Tank and Jet Tube, Referred to in Table VI.

cylinder in the form of liquid was not burnt. The experiment had been made of connecting the exhaust of a petrol cylinder to a water-jacketed coil, and the result was that all the heavier parts of the petrol were condensed and collected. It had been found that very soon after an engine was started the heavier parts of the petrol would be found in the lubricating oil in the

crank chamber. He thought that it was quite understandable that combustion would not take place in close proximity to a metallic surface, and petrol that entered the cylinder in the form of spray and touched the cylinder walls, though it was eventually evaporated, was not evaporated in time to be burnt. He believed that the experiment had been made of considerably increasing the amount of petrol passed into the cylinder, beyond the quantity that could be theoretically burnt in the air present, before smoke began to show at the exhaust; this would show that all the petrol passing through was not really ignited. A carburettor had really two functions. One was the measuring of the petrol, and the other was the mixing of it with the air. He thought that one result of the author's figures was to show what a very imperfect measuring apparatus the jet was. It varied considerably with the temperature. That would account for the fact that most carburettors could be adjusted to give a good result at a particular speed, and for the fact that it was very difficult indeed to get a carburettor that would give a good result at all speeds, and under all conditions. In most cases sufficient heat was not supplied through the carburettor. There was an objection on the part of many drivers and on the part of some makers to supplying sufficient heat, and when they found a carburettor covered with snow, it was clear evidence, he thought, that the mixture had not been heated enough, and that the petrol was not passing into the cylinder in the form of gas or vapour as it should. He once had the curiosity to take out what the quantity of petrol passing through a jet would look like in the open. The quantity that was passing through represented a jet 3 feet 6 inches high. It could be easily understood that part of that jet would reach the cylinder in the form of spray. Put in another way, the petrol in a 1 mm. diameter jet would have the same speed in feet per second as the piston of the 100 mm. cylinder it supplied. The eddy currents in the air rushing into the cylinders had been alluded to. No doubt the form of the inlet pipe had a great deal to do with the matter. Some pipes had been tried which were made in the form of a ring, so that the velocity given to the air after it passed the carburettor was not lost, but circulated round the ring when the supply to a



Type of Jet Tube in Claudel-Hobson Carburettor, Referred to in Table VI.

particular cylinder was stopped. He considered that much might be gained by developing the induction pipe on these lines.

Mr. A. S. E. Ackermann asked why the experimental jet was vertical. It seemed to him that the falling petrol would interfere with that which was coming out of the nozzle, and that it would have been better for the jet to have been horizontal. With regard to the supply of petrol to the jet from the tank, it seemed to him rather a dangerous procedure to try to keep the petrol at a dead level with the top of the upturned pipe. How could one be sure that there was no air coming down with the petrol? He (the speaker) thought that it would have been better to have had an overflow from the tank at a definite distance above the inlet of the supply pipe to the jet. Then by keeping a constant flow through the overflow one could have been sure that no air was passing to the jet, and that the head was constant. He suggested that Mr. Brewer should prepare scale drawings of the apparatus used for the experiments, which drawings could be reproduced in the 'Transactions,' so that the exact conditions under which the experiments were made might be known. Such information would add materially to the value of the figures given. With regard to Table VI. which dealt with the flow of petrol through different sized orifices and under different heads, he (the speaker) had had the coefficient of discharge calculated in a number of cases from Mr. Brewer's experiments. All present were no doubt familiar with the hydraulic formula—

$$Q = c\,\omega\,\sqrt{2gh}$$

where

- Q was the discharge in cubic centimetres per second
- c was the coefficient of discharge
- ω was the area of the orifice in square centimetres
- g was the acceleration due to gravity in centimetres per sec. per sec.
- h was the head in centimetres over the centre of the orifice.

Mr. Brewer had given all these quantities *except* c, hence it had been easy to calculate c for petrol flowing from a cylindrical orifice (such as had been used) of which the ratio of the length of the orifice to its diameter was 5 or more. The values of c for different diameters of orifice and different heads were as follows (see Table p. 227.)

These, he thought, were remarkably consistent, the maximum variation of the coefficient being from 0·738 to 0·770, i.e. about 4 per cent. The mean coefficient was roughly 0·75 which agreed well with 0·77 given by Professor Unwin on page 88 of his

'Treatise on Hydraulics' for orifices which varied from 0·032 to 0·131 feet in diameter.

Diameter of Orifice in mm.	Head of Petrol in mm.	Coefficient of Discharge = c.
0·95	30	0·744
	150	0·752
1·00	30	0·738
	150	0·750
1·05	30	0·747
	150	0·754
1·10	30	0·752
	150	0·754
1·15	30	0·768
	150	0·770
1·20	30	0·749
	150	0·757
1·25	30	0·753
	150	0·755
1·30	30	0·753
	150	0·757
1·35	30	0·754
	150	0·754
1·40	30	0·765
	150	0·767

Mr. C. H. Wingfield said that the author gave a formula for velocity in which he made it vary as the square root of the specific gravities of the liquids under comparison. The usual theoretical formula, $v = \sqrt{2gh}$, expressed the velocity of a falling body *in vacuo*, and it was a well-known fact that whether a body was heavy or light it fell at exactly the same speed. The author apparently suggested, however, that the heavier fluid, although subjected to exactly the same head, would come out at a higher velocity; so that, for instance, in the case of a tank of mercury and a tank of water, both full and both having a jet pointing upwards at the same level below the surface of the the liquid in the respective tanks, the jet of water would rise almost to its original level, but, as the heavier fluid would, according to the author's formula, come out at a higher velocity, proportionate to the square root of the density, the mercury would evidently rise higher than its original level. There might perhaps be an error in the formula as printed, or in the speaker's reading of it; he should like, however, to ask why the formula was modified by putting D over d. No doubt the author had

some idea of allowing for viscosity, and perhaps he had not noticed that it had the effect just mentioned. Only in the case of comparatively small tubes was the flow of a fluid resisted by viscosity alone. With a very thin fluid such a resistance to flow occurred in a capillary tube. With a thicker fluid, the tube might be larger. When the resistance was entirely due to viscosity, the flow was not in proportion to the square root of the head but varied simply as the head. When there was no viscosity (an opposite extreme which did not exist in nature), it would flow simply as the square root of the head, and the real velocity-ratio was somewhere between the two. Professor Unwin had pointed out that where the particles of the fluid did not move relatively to each other to any great degree, viscosity was quite unimportant. That was the case with ordinary hydraulic matters. He (the speaker) did not know whether the orifices with which the author had been dealing were sufficiently small to cause viscous flow. If they were, then the formula, taking it as the square root of the head, could not be right. If they were not, then he ought not to lay quite so much stress on viscosity. The fact that heating the fluid had materially increased the flow certainly showed that it had a good deal to do with it. He should be disposed to think that the formula was not the correct one to use, but that what was wanted was something between the two; in any case he did not know of any justification for inserting $\frac{D}{d}$ in the particular formula referred to. Possibly the author could give experimental results which had led to it.

If he was right in thinking that the author compared column 2 with column 11 in Table V., the differences were larger than he thought the author had realised. He had just run them out roughly. The first number in column 11, 0·65, was 30 per cent. larger than 0·5, the first number in column 2. The other figures were all in increasing ratios, the last one being 156 per cent. larger than 3 in column 2. He did not think, therefore, that the approximation could be described as a close one.

Table IX., column 7, gave the experimental results to which the author's calculations were intended to approximate. He found that the first number in column 8 was 58 per cent. more, and the first figure of column 9 was 45 per cent. less, than in column 7, and the other figures went on in that way, the lowest excess being 26 per cent. more than in column 7 and none were less than 18 per cent. below the experimental result. That showed that a great deal of experimental work was still needed before they could claim that they were able to determine beforehand the apertures required from data which were obtained by such experiments; but every additional experiment might be

regarded as another stepping-stone towards such a desideratum, and the author had helped matters on by making the results of his experiments public.

Mr. E. Scott-Snell called the attention of the author to what he thought must be a slip in the first part of the paper with regard to the formula for finding the vapour pressure of the petrol in a mixture of air and vapour, viz. $p = \frac{760}{1 + V\,\delta}$. δ in this case should be the absolute density, i.e. weight in grm. of 1 litre of vapour measured at 0° C. and 760 mm. pressure. This formula had been attributed to M. Sorel, but could be directly deduced from first principles as follows:—

Consider a space of V litres capacity.

Let D be the weight in grm. of 1 litre of gas (in this case air) measured at 0° C. and 760 mm.

Let d be the weight in grm. of 1 litre of vapour measured at 0° C. and 760 mm.

Let H be the pressure of the gas (in this case the pressure of the atmosphere) in mm. of mercury.

Let p be pressure in mm. of mercury of the vapour at the temperature t° C. of the mixture, and a the coefficient of expansion of gas and vapour.

Then the weight of V litres of air $= \frac{V\,D\,(H - p)}{(1 + a\,t)\,760}$ grm., and the weight of V litres of vapour $= \frac{V\,p\,d}{(1 + a\,t)\,760}$ grm.

Dividing the second expression by the first, we get

1 grm. $\left(= \frac{1}{D}\right.$ litres$\left.\right)$ absorbs $\frac{p\,d}{D\,(H - p)}$ grm. of vapour.

Hence 1 litre absorbs $\frac{p\,d}{H - p}$ grm. of vapour,

or V cubic metres absorb $\frac{V\,p\,d}{H - p}$ kilogs. of vapour.

From which we have, if 1 kilog. of vapour is absorbed in V cubic metres of air,

$$p = \frac{H}{1 + V\,d}$$

where d is the same as δ in the equation given by Mr. Brewer.

He would like to ask the author if he could tell him where any information as to the vapour pressure curves of various

petrols could be found, if any such details were published. He had done a considerable amount of reading but had failed to find any figures of practical value. He would also like to ask the author whence he obtained the figure for the latent heat of evaporation of petrol as 1·41 of the total heat of combustion, and if any details of experiments on this point were published. If not, could he specify the particular petrol to which this figure referred.

Mr. E. G. Beaumont said that the author suggested that it was inconceivable that the whole of the fuel could be evaporated completely during $\frac{1}{40}$ of a second, and the suggestion was that $\frac{1}{40}$ of a second was the probable period during which carburation might be commenced and completed. He (the speaker) had roughly taken the probable period occurring in the average engines now used and from the time the air passed the carburettor jet to the time the compression was complete, which he thought might be regarded as the period of carburation, would be about $3\frac{1}{2}$ times $\frac{1}{40}$ of a second. The time was still short, but it was considerably longer than the time during which combustion occurred, and (as shown by results of running) was completed. A little further on the author called attention to the heating of the fuel before it issued from the carburettor jet. He (the speaker) would like to know whether any advantage resulted from heating the fuel, because as the specific heat of the fuel was low, the assistance towards the equalisation mentioned, or the correction of the reduction of temperature, would be very small.

With regard to the experiments as to flow, he was quite aware that the author had tackled a very difficult problem, and one which all who had had to do with jet carburettors realised was extremely difficult. He found that the sizes of jets given were large, inasmuch as sizes varying from 0·6 to 0·8 of a millimetre were as commonly used as those of larger sizes. He had not closely inspected the jet of the Claudel-Hobson carburettor, and he did not quite know whether the experiments included others, or whether the jets were all the perfectly plain single-opening form used on 90 per cent. of the engines at the present day. Both the jets referred to in the paper were of what might be called special form.

Mr. R. W. A. Brewer said that the jet was a plain single-hole jet.

Mr. E. G. Beaumont asked whether the jets referred to in connection with the Claudel-Hobson carburettor were those with the plain single hole.

Mr. Brewer stated that they were. All the tests were through a plain single parallel hole, an ordinary type of jet.

Mr. E. G. Beaumont said that he would like to ask whether any experiments had been made with jets of a smaller size, and if any data had been collected to show whether, with the small jets which were commonly used, the square root of the head formula which had been applied seemed to apply equally well as it did with larger jets.

As to the flow becoming so erratic through the small jets or small orifices it suggested that the viscosity effect might become more rapidly appreciable or more important with the reduction of the size of the jet. He thought it was reasonable to suppose with the formula on the blackboard, which was applicable to the flow of water through pipes of one inch diameter upwards, that viscosity and the influence of variation in the character of fuel and so forth would become apparent. While recognising the difficulty of measuring the area of the openings in the Longuemare carburettor type of jet he would be interested to know whether any figures had been collected to show what the petrol consumption was under the rates of air flow and degrees of suction pressure shown in the table applying to the Longuemare carburettor, so that some comparison could be made between those figures and similar figures applying to the Claudel-Hobson carburettor. For instance, in Table VIII., column 12, at 1250 revolutions of the engine there was an air velocity of 251 feet per second, and in Table V., column 10, the air velocity at the same engine speed was shown to be 176 feet per second. As the consumption result appeared to be made on the Claudel-Hobson carburettor with very high air velocity, it would be interesting to know what the corresponding consumption was with the Longuemare type.

Referring to the road tests, he noticed that the tests were made over periods averaging about 10 to 12 minutes, and over distances of about 2½ miles. From his own experience in making large numbers of tests for various purposes he had found great difficulty in obtaining results which could be depended upon over such short distances. He should therefore like to ask if the author had taken special precautions to avoid error in those short trials.

At the end of the paper there were certain figures of consumption of benzol. There were no definite statements as to what the atmospheric temperature was at the time or as to whether the engine was started from cold. He believed that in one case the engine was started from cold. He asked that, because he had knowledge of the running of an engine with benzol where great success had attended the use of benzol as

long as the atmospheric temperature was not less than 50° F., he believed; he was speaking from memory now. When the temperature fell below that, there was an increasing difficulty in starting, until finally the use of benzol was given up and petrol was used. The engine was a 4-cylinder 15–20 H.P. Panhard and Levassor, with the usual carburettor.

Mr. J. Johnston said that Tables I. and II. would quite upset the claims of some manufacturers of so-called automatic carburettors. In Table I. there was a range of temperatures, but the figures given were not of much practical value, for the simple reason that no one thought of heating the petrol before it was used in the carburettor. He had done so with a paraffin engine, but he had found that the benefit obtained was not worth the trouble involved in keeping the temperature constant. Taking an average range of temperature of 30° as being the ordinary working winter and summer conditions, he found that the change in specific gravity was about 2 per cent., but the change in the flow was about 20 per cent. There was no (so called) automatic carburettor in existence, which, if set for one condition, would work automatically for another.

He was rather disappointed to find that there was nothing in the paper with regard to the inertia and momentum effects on the jet, because he was convinced that the control of those was of far more value in practical carburettors than the simple flow through the nozzle. He was quite convinced that there was not an internal combustion engine with a multiple cylinder that would give off practically the same power in all the cylinders by reason of these effects. He had tested a good many 2-cylinder engines with opposite cranks. There was not a single one which gave the same power in both cylinders. The first cylinder to fire was the weaker; the second was the stronger, for the simple reason that the first cylinder started the momentum in the pipe, and the second one got the benefit of it. The same thing applied more or less to 4-cylinder engines. In some engines the best carburettors in existence, taking them all round, would not work all the cylinders equally, because of the design of the induction pipes, bad working often having nothing whatever to do with the carburettor. The momentum effects in the pipes crowded the greater part of the mixture into one or more of the cylinders, while the others were starved. Thus they found many cylinders corroded or carbonised, while others were perfectly clean. The same thing applied to sparking plugs. Some cylinders were continually getting into trouble, and others would work and keep perfectly

clean. That had nothing to do with the carburettor or the flow of fuel; but it depended on the care that was taken of the gases after they were formed. There was a remark somewhere in the paper about the jet designed to give a mixing effect. He was convinced that there was not a jet in any carburettor that gave a sufficiently good mixing effect. He had a carburettor on a small single-cylinder engine that had been running in London, day after day, using ordinary paraffin. A carburettor that would use paraffin would not give any trouble if it were using benzol, or petrol, or any other spirit. In connection with this carburettor he had carried out a large number of tests to get a good mixing device. On one occasion he used some corrugations in the carburettor and he found that they helped him. In the next one that he built he put more corrugations, with the idea that there could not be too much of a good thing, but he found, unfortunately, that the result was not what he had anticipated. Instead of getting better mixing, he found that he had formed a separator, and therefore got a worse result. Still he kept hammering away until he found what did give a good effect. It was a very easy matter to put in a number of flanges or baffles which would give mixing effects, but most of them also gave resistance, and resisted the passage of the mixture to the engines. He found in mixing that the best thing was to get cross currents, if possible, so that one current, striking across another, mixed up the vapour without coming into contact with the metallic surfaces. This ought to be done, if possible, in a hot chamber.

Mr. Worby Beaumont said that the inertia effects which Mr. Johnston had found important in his practical experiment were those which were referred to by Professor Smith, when he spoke of the fact that the acceleration of the liquid had been left out of consideration in many of the papers and discussions.

Mr. E. J. H. Norman wrote, saying that much praise was due to Mr. Brewer for the valuable data that he had obtained from his experiments, which in themselves no doubt would be useful, but not so useful as actual results taken from the condition of the vapour at the time of entering the cylinder on the suction stroke. No doubt there was a great percentage of fuel lost between the carburettor and the cylinder itself. Taking into consideration the suction stroke of the engine, this might be used to show the velocity of the fuel, and it was found that at a little distance before the end of the suction stroke this velocity must drop, and that it was beyond this point that we get unvaporised fuel coming into the cylinder, as it was under-

stood that the greater the velocity of the fuel the quicker the vaporisation took place. The highest point of velocity on this curve being ascertained, this was the point at which the supply of fuel should be instantaneously cut off, and this should take place as near to the walls of the cylinder as was possible by a mechanically controlled valve, as it was suggested by most engineers that dirty valves and plugs were due to partially vaporised petrol. This was the portion of the fuel which entered the cylinder after the highest point of velocity had been reached, and through not entering at the same rate as the remainder of the fuel, it fell in a half-vaporised state on to the piston or the walls of the cylinder. Some would argue that, by cutting off the supply of fuel earlier, power was lost, but it seemed possible that the fuel which remained in the cylinder in an unvaporised state during the compression stroke did not add to the power at the time of explosion. Most likely the explosion helped to partially vaporise it, and it afterwards found its way through the exhaust valve with the exhaust gases, and had no doubt a great deal to do with the cause of dirty cylinders. It would be interesting to have experiments carried out showing the condition of the vapour during the full length of the stroke.

Mr. R. W. A. Brewer, in replying to the discussion, said he wished to point out a misconception upon the part of Mr. Worby Beaumont, and draw his attention to the top of page 213, the last three words of the first sentence, "the average motorist," as he wished it to be clearly understood that the paper was specially written for the purpose of pointing out to the ordinary user of a car how it was possible to obtain something reasonably definite with regard to what takes place under various conditions of air velocity past the jet. He pointed out that the large amount of research work which had been carried out by designers of different carburettors was in no way to be confounded with the results which are published in this paper, and it is only natural that those who had experimented for their own ends, and in order to enable them to design certain types of carburettors, should keep their results private, so that they and they only should benefit by their particular work. As the author had not been able to obtain any results of the experiments of others upon this subject, he conducted a large number of experiments, the results of which had been embodied in the paper, for the use of anyone who chose to use them, and not in any way to dictate to the world at large, and designers especially, with regard to the action of a jet. Mr. Worby Beaumont appeared to have some misconception regarding the

shape of the jet tubes, and also took exception to the type of carburettor in which they were tried. The primary object of the paper was, as its title indicates, to determine "the flow of liquid fuel through carburettor nozzles," and not the behaviour of petrol in any particular carburettor. The author obtained the best nozzles he could for the purpose, and was fortunate in selecting a series of carefully constructed tubes which were made in France. These tubes had a plain hole about 1·5 mm. in diameter, drilled up the centre, the final aperture being about 5 mm. long, and drilled to gauge varying from 0·95 to 1·4 mm. in diameter. The length of this small bore was scarcely sufficient to be affected by capillary attraction as had been suggested by one of the speakers. It was in all respects similar to the plain single-hole jet tube used in the majority of carburettors, and not, as Mr. Worby Beaumont suggested, a peculiar type used in a small percentage of carburettors. There might have been some misconception as to the object of the surrounding sleeve which was perforated. The effect of this sleeve was to minimise the local suction at the jet orifice, this orifice being situated in the strangler chamber of the carburettor. At certain positions of throttle opening there was a difference of suction-pressure at the orifice of the jet tube and at the carburettor air inlet. This sleeve was designed to minimise the local suction under such conditions, as, for instance, if the engine speed were 600 r.p.m. This speed might occur under two different sets of conditions, first when the engine was working up to its full power with the throttle open (as when ascending a hill), secondly when the throttle was partly closed and the car-speed thus regulated upon the level. In each case practically the same amount of air passed through the carburettor to the engine. There was naturally slightly less air due to wire-drawing when the throttle was partly closed, amounting to say 10 per cent. The action of the sleeve was to proportion the petrol flowing, to the air passing, so that the local suction at the jet orifice, which was produced by the partial closure of the throttle, would not affect the flow of liquid from the nozzle to any great extent. This was graphically demonstrated on curve G, where the chain dotted and full line curves marked "Suction at jet" lie very close together throughout the ordinary working range of speed. With reference to Mr. Worby Beaumont's remarks as to the rates of flow under steady heads, such as in the author's experiments with the pressure system, as compared with intermittent and varying suctions due to the engine, the object of the author in using these jets in a carburettor as a separate experiment was

to compare the results obtained in each case. For this reason, during experiments upon the road, tests were made with the engine speed fairly constant over given distances, and also with the engine speed varying over longer distances by amounts represented by 15 to 22 miles per hour car speed. An average was taken of the readings in these cases which was plotted in curve G, marked "Suction on strangler." After this curve was plotted, the theoretical curve was placed in its position upon the chart, and it would be seen how very nearly these two curves agreed. However, at a speed of 900 r.p.m. the actual results fell below the theoretical ones, which was a point in their favour, the tendency always being for the suction to increase disproportionately at higher speeds, as was shown in the two chain dotted curves representing light loading (curve G).

The author agreed that final adjustments would always be necessary whatever system were employed for the preliminary determination of the size of the jet, as the conditions of working varied enormously from moment to moment and even from day to day, so that it was impossible to determine with any degree of accuracy what exactly would take place in a jet-suction type of carburettor. For this reason the author had always strongly advocated the adoption of a mechanical type in preference to any other. The particular carburettor used was chosen because it had no moving parts, such as automatic air inlets, which were out of control, and the behaviour of which might upset the experimental results, and although the actual results obtained, as far as the suction was concerned, varied from the calculated results on Table V., the amount of difference, which was represented by the suction, did not imply that the flow from the jet, as found experimentally and calculated, varied through the same limits. Reverting to Table V., columns 3 and 12, the suctions were calculated at the two ends of the taper entrance of the Longuemare carburettor. The experimental tube was inserted about the middle of this taper, so that one would have expected to have obtained a reading which was about the mean of the two calculated ones. This for 600 r.p.m. gave a flow taken from curve C for the experimental suction of 1 inch = 0·42 gallon per hour flow, and for the calculated value, being the mean of columns 3 and 12, giving a flow of 0·48 gallon per hour; this was for a jet 1·05 mm. diameter. For a speed of 820 r.p.m. the flow from the experimental suction was 0·53 gallon per hour, and from the mean of the calculated columns 3 and 12 it was 0·65 gallon per hour. However, it was pointed out that, as the suction increased, air came in through the top of the carburettor

over the jet, on account of the presence of a regulating device which was fitted to this particular carburettor.

In reply to Professor Robert H. Smith, the author said the effect of the variation of resistance to the flow of air at different speeds was not taken into account, as he considered that there were more serious errors which were liable to affect the results, and that it would be a complication giving little practical effect to the results obtained. The particular shape of jet tube was shown in the sketch (page 225). The queries in relation to the same have been replied to in dealing with Mr. Worby Beaumont's remarks.

Mr. Veitch Wilson dealt with the action of atomisation in causing deposits which were often attributed to the lubricating oil. It had generally been found that if the fuel contained heavier fractions which were not usefully burnt in the cylinder, these fractions passed right through the engine into the exhaust, and might be condensed there. If the fractions were due to insufficient distillation, and contained oily matter, it would adhere to a cooler surface, and there crack and leave a tarry residue. This was particularly noticeable in the case of mixtures of paraffins and lighter fractions. With regard to viscosity, it was interesting to note the different behaviour of oils under increased temperature, some of which lost their viscosity very much more rapidly than others. Throughout the range of temperatures covered in curve B in the paper, this loss was fairly constant, but it had a considerable effect upon the flow of a liquid such as petrol through an orifice of the size usually employed in a carburettor. The instrument which the author had used had a small aperture which could be adjusted within certain limits, as no ordinary instrument designed for lubricating oil would suit such a fluid as petrol.

Mr. Douglas Leechman considered that the author had not proved anything conclusively, and he (the author) agreed that no work of this nature would ever be absolutely conclusive, as the subject was far too complex to be bound within the limits of any hard and fast rules or formulæ. No one set of conditions could hold good in all cases where the proportion of fuel to air depended upon the suction produced by the engine. The question of the range of explosive mixture was important, and although not dealt with by any of the speakers, it was alluded to. It was the actual explosive range, the limits of which varied over only about 7 per cent., which determined the limits of accuracy in any carburettor arrangement. The author had not experimented to any great extent with notched nozzles, although

he was aware that many devices of this kind had been tried to regulate the flow of petrol before reaching the orifice.

With reference to Mr. Lyons Sampson's remarks, there was no doubt that the form of inlet pipe had a very considerable effect upon the carburation, and the author understood that for some time experiments had been carried out by Colonel Holden and Mr. Duckham upon this subject, the results of which would no doubt greatly add to our knowledge of carburation.

In replying to the discussion upon the author's previous paper, Mr. Duckham had made some very interesting remarks upon the passage of unconsumed fuel through the engine, and the author's results confirmed those which had been alluded to by Mr. Duckham.

Mr. Ackermann wished to know the direction in which the petrol issued from the jet. The tube was directed upwards and slightly inclined from the vertical, so that none of the liquid fell out on to the nozzle, which might have impeded the flow of the issuing jet. Care was taken to prevent air entering the pipe leading to the jet. This pipe stood about 10 mm. from the bottom of the test tank, and one edge had a small vertical projection (as shown in the illustration on page 224), which indicated when the level of the liquid was near the top of the tube, so that the flow fed into the tank could be regulated. It was interesting to note that Mr. A. S. E. Ackermann's calculation of the coefficient of discharge for the jets appeared to show that the author's experiments were consistent.

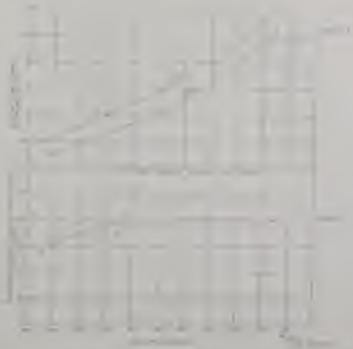
Mr. C. H. Wingfield referred to the formula on page 202, but a study of the letterpress at the bottom of page 201 would show that this formula was given by M. Edmond Ledoux in connection with calculations for benzol, as was the matter at the top of page 202, which was referred to by Mr. Scott Snell. Half way down this page it would be seen that the author did not entirely agree with this formula, and it was very interesting to note that his opinion was confirmed by other speakers at the meeting. The author had in many cases taken into account the square root of the densities of fuels under comparison, as the density affects the pressure of the fuel at the orifice, which pressure had some bearing upon the frictional resistance of the liquid issuing through the orifice. He agreed with Mr. Wingfield that as far as the equation $v = \sqrt{2 g h}$ was concerned, neglecting viscosity or friction, the density had no bearing upon the subject. As the temperature increased the viscosity decreased, which was shown by the author in his argument upon benzol and petrol at the higher temperatures.

With regard to the decrease in the proportional suction, Table V., column 2, these questions had already been answered, but a careful comparison between Table V. and Table VIII. brought out many interesting points with regard to the fuel supplied to the engine in each case. The two carburettors exhibited had been in use for a considerable time in connection with the same engine, and although the air velocities were widely different, practically the same amount of fuel was fed to the engine in each case. Mr. Scott-Snell had referred to Sorel's formula. This work was in French, and the author did not know of a translation of it, or of any work of a similar nature in English. Vapour density was not defined in the source from which the author took this formula, and he had assumed that it was in standard English notation.

Mr. E. G. Beaumont had stated that $\frac{1}{40}$th second was an unreasonably short time, but in the case of a modern high speed engine, running at something over 2000 r.p.m., the whole time of operation of a revolution was of this order. The object of heating the fuel before it issued from the nozzle, was to decrease its viscosity and obtain a more regular flow. In the case of a cold fuel, the author found that the issuing jet tended to break up into beads when the jet was small. A jet, 0·95 mm. diameter, the smallest used by the author, was considered by him to be the minimum practical size, as under a pressure head corresponding to 220 mm. of water it passed only about 1 gallon of fuel per hour, and under ordinary running conditions 0·6 gallon. At these lower pressures, such a jet was extremely erratic in its behaviour unless subjected to frequent increases of suction, which could be done by racing the engine. The relative consumption with the two carburettors was about the same under ordinary conditions, but the Claudel-Hobson was more satisfactory in traffic and under rapidly varying demands, owing to its better regulation of the proportions of the mixture. The distances quoted in the paper to which Mr. E. G. Beaumont took exception, were given to show a certain set of conditions. The author's tests had been conducted over many thousands of miles, and the mean results were used as explained in his previous paper. The dimensions of the engine were four cylinders, 90 mm. diameter by 110 mm. stroke, with valves on opposite sides and plugs in the middle of the combustion chamber. Synchronised electric high tension ignition was used.

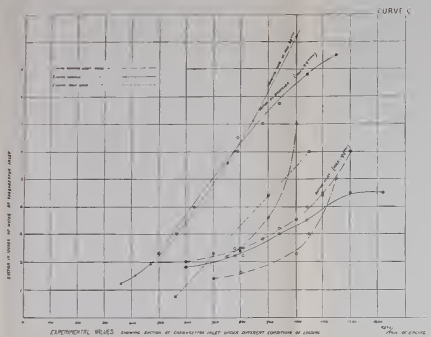
Mr. Johnston referred to the heating of petrol. This had been replied to in answering the previous speaker, as also his remarks with regard to the shape of the induction pipe. It was

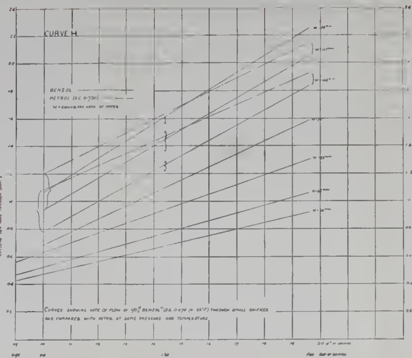
interesting to note that Mr. Johnston had produced a carburettor for use with paraffin, especially if it were regulated by engine suction, as this had baffled designers for many years, owing in some degree to the small limits of explosive range of paraffin and air mixtures, and the probability of small derangements upsetting the whole system.

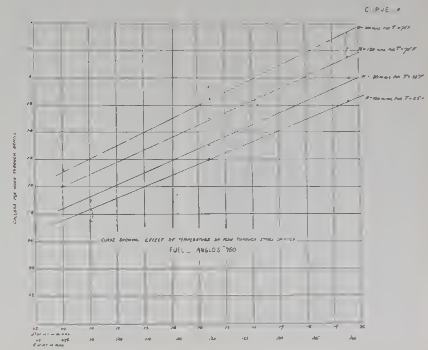


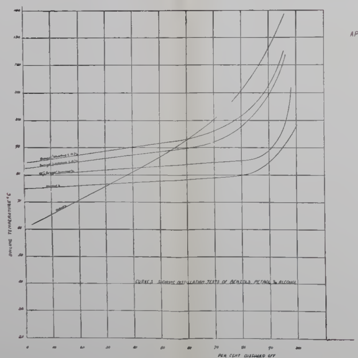
THE FLOW OF LIQUID FUEL THROUGH CARBURETTOR NOZZLES.
BY ROBERT W. A. BREWER.

PLATE II.

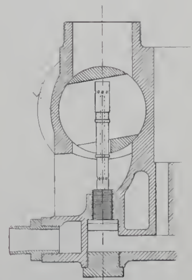




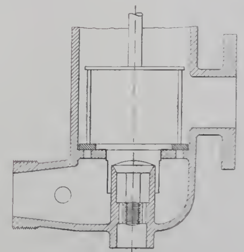




APPENDIX A



CLAUDEL - HOBSON CARBURETTOR



LONGUEMARE CARBURETTOR

December 7th, 1908.

JOSEPH WILLIAM WILSON, PRESIDENT, in the Chair.

MECHANICAL FLIGHT.

BY

HERBERT CHATLEY, B.Sc. (Eng$^{g.}$) Lond.
M. AERO SOC. GT. BRIT.

PRESENT POSITION.

THE recent records made by Messrs. Wright, Farman, Delagrange and Bleriot, together with the gradual accumulation of testimony in favour of mechanical flight, have finally disabused both the public and experts of the notion that aviation is a dream.

Many engineers from time immemorial have tackled the subject without success, and there was every reason for the sceptical attitude which has prevailed until the last few years. It is now evident that mechanical flight was impossible before science and engineering skill in the nineteenth century had so perfected the heat engine that considerable power was obtainable with but little weight. The present improved aspect of affairs must not, however, blind us to the fact that much has yet to be done. The most successful machines now in existence show serious defects, cannot be manipulated in troublesome weather, and have every part so light that at all times they are on the brink of collapse. It rests with mechanical engineers to design a stronger machine without losing efficiency. In the course of this paper the author proposes to indicate certain points in which improvement is desired, and at the same time he has endeavoured to include a sufficiency of the theoretical and experimental knowledge available on the subject to enable a would-be aviator to construct a simple type of machine.

It cannot be too strongly realised that existing information is defective, and a few words as to research may be useful.

NECESSITY FOR RESEARCH.

It will be shown in the course of this paper that the whole question of mechanical flight depends upon a knowledge of the

manner in which air reacts against solid bodies moving through it. A large number of researches have been made during the past 150 years, but even yet exact information is lacking on the majority of points.

Furthermore, mathematical analysis has not been sufficiently developed. A few great mathematicians (including Lords Kelvin and Rayleigh) have devoted some attention to the matter, but the author is not aware that any mathematician worthy of the name has considered it worth while to make an exhaustive study of the question, although it must be recognised that the recent advances in the theory of hydrodynamics form useful auxiliaries to the study of aërodynamics.

Prevailing Types.

Considering then the somewhat vague data available, it is not surprising to find that invention has proceeded in several directions. Many unique types of machines have been projected, but only a few have shown a claim to be considered as possessing further qualifications than uniqueness. Three classes may be clearly distinguished :—

a. The Aëroplane, or Glider.
b. The Helicoptère, or Screw machine.
c. The Ornithoptère, Aviplane, or bird machine.

Aëroplane.

This most successful type is practically a kite, sustained by the mutual action of three forces: its weight, the reaction of the air to which it has relative motion, and the thrust or pull of a propeller. If the latter be acting horizontally, it is clear that the vertical component of the air reaction must be equal to its weight, so that a certain minimum relative velocity is indispensable.

Helicoptère.

This machine has proved practically successful, but is subject to certain disadvantages. It consists of a framework sustained by the pull from a number of propellers, forcing the air downwards. Since, except when rising rapidly, there is enormous slip, this arrangement is mechanically inefficient.

Ornithoptère.

Innumerable attempts have been made to imitate bird flight, but very few have shown any measure of success. One machine (Bastin's) has made short flights, the wings automatically per-

forming a periodic motion resembling that of a bird's wing. In this type the conditions are so complex that it does not seem at all promising.

Relative Advantages of the Three Types.

Although the values of these three types cannot be properly appreciated until the fundamental principles have been considered, yet certain preliminary comparisons may be made. Both the ornithoptère and the aëroplane labour under the disadvantage of requiring relative motion as a whole, so that it is (in the absence of a convenient wind) impossible for them to remain stationary over any point on the ground. This the helicoptère (in the absence of wind) can do. Furthermore, both the aëroplane and ornithoptère have great difficulty in gaining and losing this relative motion at starting and alighting, so that here again the helicoptère is superior. On the other hand, the aëroplane gains in lifting power by the use of a wedging action, and a more economical use of the propeller.

Brief History of the Theory.

The nature of fluid resistance has been investigated for many years, and the general principles are to be found in Newton's 'Principia.' The ballistic researches of Hutton and Robins at the end of the eighteenth century, first clearly showed the quantitative value of air resistance, and their work is still valuable. On the hypothesis deducible from Newton's work, Messrs. Navier and Gay-Lussac early in the nineteenth century formulated a theory of flight which showed that great power was necessary, and this notion held sway for many years after, so that little progress was made with the subject, flight being deemed impracticable. Experiments by Wenham and Browning in the eighties, together with Langley's researches in America and Maxim's in England, clearly showed the fallacy of this idea. Pénaud in 1876 first gave the mathematical theory of the aëroplane, which had been conceived by Henson in 1840. The late Mr. Froude, Lord Rayleigh, and Professor Bryan developed this theory, and in 1903 the last-named produced equations of stability for the aëroplane. Two years later Captain Ferber, of the French artillery, amplified these equations to find the conditions of lateral stability and the form of the trajectory, and quite recently Mr. Lanchester has done similar work. Professor Fitzgerald and Lord Rayleigh have given some attention to the ornithoptère, and Professors Pettigrew and Marey at an earlier date arrived at several important conclusions respecting bird flight. The Helicoptère has not received very much attention, but the cognate work of the

late Mr. Froude and his son on propellers has a most important bearing on the matter. Mr. Alexander, Sir Hiram Maxim, and several other engineers have made researches on the subject of air-propellers.

Theory.

*Resistance of Surfaces and Solids.**—A certain resistance is experienced when any body is moved through the air, depending on the form of the body and the relative speed. If the air is abruptly parted the sudden alteration of its relative momentum causes a thrust on the body; its friction against the body produces further resistance, and the partial vacuum at the rear (due to the air not immediately returning) causes still more resistance. The air enters this rear space in a series of whirls or eddies, the kinetic energy of which must be supplied by the moving body. Hence we must consider the front form, the surface, and the rear form of the moving body. All the effects are, at the speeds commonly occurring, nearly proportional to the square of the speed.

If a thin but rigid plane be moved perpendicularly to itself with a speed of V feet per second, it will be subjected to a dynamic resistance and also to a negative pressure due to the whirling behind. The skin resistance will be negligible except when the dimensions are very great. The dynamic resistance depends on the quantity of air affected, which again varies with the area, so that we may write—

$$P = k\left(1 + \frac{1}{n}\right) S V^2 \quad . \quad . \quad . \quad (1)$$

Where P is the total pressure in lb., S is the area in square feet, V the speed in feet per sec., and k and n are constants; k is the mass of a cubic foot of air divided by 2, and is equal to about ·0012 at normal temperature and pressure; n is the ratio of the dynamic to the negative pressure, and is generally rather more than 2, so that $k\left(1 + \frac{1}{n}\right)$ varies according to different experimenters from ·0013 to ·0017. Langley's value ·0017 is frequently used, so that we have—

$$P = k S V^2 \text{ (where } k = \cdot 0017) \quad . \quad . \quad (2)$$

If the plane be turned so that it make an angle γ with the direction of motion, the dynamic action is no longer symmetrical,

* See Lamb's 'Hydrodynamics,' Lanchester's 'Aërodynamics,' also an article by the author on the 'Stream Line Theory in Relation to Aërodynamics,' in 'Aëronautics,' August 1908.

skin friction becomes important, and negative pressure decreases. Many rules have been given for this case, but except for very small (say less than 2°) and very large angles (more than 40°) the following rule will serve:—

$$P = 2\,k\,S\,V^2 \sin\gamma \quad . \quad . \quad . \quad (3)$$

As the surface becomes nearly coincident with the direction of motion P decreases, but there is a certain residual resistance due to edge dynamic action and skin friction. Lanchester makes this approximately

$$F = \frac{2\,k\,S\,V^2}{20} \quad . \quad . \quad . \quad . \quad (4)$$

where F is the total resistance in lb.

[The author is responsible for this formula.]

This means that the coefficient of skin friction is upwards of 5 per cent. of the coefficient of resistance. There is some difference of opinion as to this, but the value will serve.*

Curved surfaces experience analogous resistances when inclined so as to present a *definite* convexity or concavity forwards, the coefficient being rather larger. If such surfaces have their chords in the direction of motion, they will be subject to skin friction, and will also experience an upward or downward thrust according as the convexity is beneath or above, provided that the curvature is easy so that the air may stream into the concavity. Surfaces laterally great experience more thrust than those whose major dimensions are in the direction of motion, the ratio of thrust per unit area varying about 30 per cent. above and below that on a square surface.

The resistance of air to solids in motion is similar to that of water, but in the decreased ratio of the density of air to water (about 1 : 800).

Centre of Pressure.

The dynamic resistance is not symmetrical, the resultant pressure being ahead of the centre of area. More information is required as to this displacement. For planes inclined at an angle γ to the direction of motion, the following rule, given by Joëssel and Avanzini, is much used:—

$$\Delta = 0 \cdot 3\,(1 - \sin\gamma)\,L \quad . \quad . \quad . \quad (5)$$

where Δ is the distance in feet from the centre of area to the

* See Baden Powell's 'Practical Aërodynamics,' Langley's 'Experiments in Aërodynamics,' and the author's book, 'The Problem of Flight.'

centre of pressure, and L the length in feet of the plane in the direction of motion.

Turnbull ('Phys. Review,' xxiv. March 1907) contests this rule, and states that his experiments indicate that when γ is less than $18°$, Δ simply varies with γ, so that when $\gamma = 0$, $\Delta = 0$. For surfaces having a convex underside or concavity in front and convexity at the rear (both on the underside), he gets a law similar to, but in excess of, Joëssel's. He maintains that these two types of surface only are stable.

As this quantity enters into all the stability formulæ, further experiment is urgently required.*

Energy Required for Flight (Aëroplane).

Since the normal pressure varies as the area of the plane and the square of the speed, the component of this in the direction of motion will similarly vary. Thus, if the thrust is in the direction of motion we have R the resistance of the plane in lb.

$$R = P \sin \gamma = 2\,k\,S\,V^2 \sin^2 \gamma \quad . \quad . \quad (6)$$

and if a further resistance $C\,V^2$ be allowed (where C is the projected area in square feet of the car at right angles to the direction of motion) for the car and framework, we have

$$\begin{aligned} H &= (R + C\,V^2)\,V \\ &= (2\,k\,S \sin^2 \gamma + C)\,V^3 \quad . \quad . \quad (7) \end{aligned}$$

where H is in foot-pounds per second.

Hence the power required appears to vary as the cube of the velocity. γ, however, is not necessarily constant, so that we may diminish the power by decreasing γ, always remembering that C is invariable. The limiting value of γ is determined by the weight, for the vertical thrust must never be less than the weight. If the direction of motion is horizontal, then we have

$$W = P \cos \gamma = 2\,k\,S\,V^2 \sin \gamma \cos \gamma \quad . \quad (8)$$

where W is the weight in lb., so that V being known, γ can be computed, or *vice versâ*. It will follow from this that if a certain starting value for γ is assumed, the value V, found from equation (8), will be the lowest soaring speed, i.e. the starting speed required.

By substitution between (7) and (8) the speed corresponding to a given power and angle may be obtained, or the power required to drive the machine at any particular angle and speed.

* See Turnbull's paper, also Kummer 'Berlin Akademie Abhandlungen,' 1875-6; Joëssel 'Génie Maritime,' 1870; Langley 'Experiments in Aërodynamics,' Moedebeck's 'Pocket-Book.'

If the machine be rising, so that the line of motion is inclined at an angle θ to the horizon, then (8) becomes

$$W = P \cos(\theta + \gamma) = 2\,k\,S\,V^2 \sin\gamma \cos(\theta + \gamma) \quad . \quad (9)$$

By substitution between (7) and (9) we can find the power, speed, and angle in terms of one another in the new circumstances, which are the most adverse that have to be considered.

Power Required for a Helicoptère.

This will follow at once from a consideration of propeller thrust. For if T be the thrust in lb. of a propeller, under given conditions as to speed and slip, then in a helicoptère

$$W = n\,T \qquad . \quad . \quad . \qquad (10)$$

where n is the number of propellers.

The ornithoptère will be discussed later.

Efficiency of Propellers.

Experiment has generally shown that, subject to correction for the difference of density, an air-propeller is almost identical in its action with a marine propeller. The thrust is proportional to the area of the blades and the square of the speed, and the power varies as the cube of the speed. There is a diminution of thrust with a decrease of slip, and both power and thrust increase with the diameter of the propeller. There is no necessity to present here the general conclusions as to propellers, which will be found in Mr. Froude's papers in the 'Trans. Inst. Naval Architects,' and in text-books on naval architecture and marine engineering. There is, however, one respect in which the action of a propeller in air differs from that in water, viz. the feed. Owing to the small inertia of air, a propeller, revolving on a fixed axis in air previously stationary, rapidly ejects air by axial propulsion and centrifugal force, and tends to surround itself by a vortex of air, with a consequent diminution of the thrust to almost zero. This is the reason for the lack of success in experiments which have been made on lifting screws for helicoptères. On the other hand, an axial or transverse flow caused by motion of the axis of rotation will supply the propellers with the necessary fresh air, and consequently we find that the smaller the slip (i.e. the greater the advance) of the screw the greater its efficiency. Similarly, in helicoptères moving laterally there is more lift.

For a sustaining screw not rising (i.e. with 100 per cent.

slip) the author has deduced the following formula for the thrust (see 'The Problem of Flight,' p. 9)—

$$T = 0 \cdot 1\, r \sqrt[3]{H D^7} \quad . \quad . \quad . \quad (11)$$

where T is the thrust in lb., r the revolutions per second, H the horse-power, and D the diameter in feet of the propeller. This is based on the assumption that the area is that required by the conditions as to power, diameter, and speed. The following rule for the projected area must be applied—

$$A = \frac{4}{\pi r} \sqrt[3]{\frac{H}{D^5}} \quad . \quad . \quad . \quad (12)$$

where A is the ratio of the projected area to the disc area.

These rules are based on Mr. W. G. Walker's experiments with fans, particulars of which will be found in Mr. Innes' book on 'The Fan.'

The thrust per H.P. obtained with the best forms of propellers varies from 20 to 60 lb., 40 being the common maximum. The mechanical efficiency, as in the case of marine propellers, rarely rises above 50 per cent., the best results being obtained with a minimum of slip. This alone gives the aëroplane a superiority over the helicoptère.*

Stability of Gliders.

We have seen that the centre of pressure is ahead of the centre of area, and that the distance between these two depends on the angle γ. If then the angle and the normal pressure are constant, the turning moment of the pressure about the centre of area is also constant, and may be balanced by shifting the centre of gravity until it lies over the centre of pressure. Seeing, however, that neither the angle nor the resistance is absolutely constant, it might be supposed that stability was impossible. That this is not so has been demonstrated by Professor Bryan and Mr. Williams in a paper read before the Royal Society in 1903, and by Captain Ferber in an article in the 'Revue d'Artillerie' (Nov. 1905). In the latter it is shown that an aëroplane is longitudinally stable, if two conditions are satisfied.

(1) That the longitudinal radius of gyration about an axis through the centre of gravity does not exceed

$$\sqrt{\frac{P}{37\,b}} \quad . \quad . \quad . \quad . \quad (13)$$

when P is the weight of the aëroplane in kilogrammes, and b the overall width of the machine in metres. The radius of gyration is here measured in metres.

* See the author's paper to the Aëronautical Society, October 1908.

(2) That the centre of gravity falls over the centre line between two points, one a little ahead of the centre of area of the sustaining surfaces, the other near the forward edge of the aëroplane. The exact values of these positions depend on the characteristic magnitudes of the machine through a series of somewhat complex equations, for which the papers referred to should be consulted. It must be recognised in this connection that the probable inaccuracy of Joëssel's formula invalidates the accuracy (*not* the method) of the values given by Captain Ferber in this paper.

If the centre of gravity coincides with one of these points, the machine is subject to two oscillations of long and short periods respectively, any increase of which will lead to collapse.

The behaviour of a machine running with a certain initial speed is then somewhat as follows. The continued resistance tends to retard the machine, and to cause the velocity to fall below the soaring limit, and the weight (in front of the centre of area) causes the front to dip. The gravitationally acquired velocity causes a forward acting pressure on the surface, so that if the machine is stable (in accordance with the above conditions), it settles down into a condition in which the resistance due to the resultant velocity just balances the component of the weight in the direction of motion. Pénaud has shown that the angle between the plane and the direction of motion (trajectory) ("l'angle d'attaque") is half the angle between the trajectory and the horizontal, when the trajectory is such as to give the greatest travel.

This condition is satisfied when

$$\tan \gamma = \sqrt{\frac{C}{2 k S}} \qquad [\text{see } (7)] \quad . \quad . \quad . \quad (14)$$

If from any cause the machine loses velocity, it will drop and gain kinetic energy by loss of potential, until its velocity is that required. On the other hand, if accidentally its velocity increases, it will rise, to lose kinetic energy by gaining potential energy. It is this exchange in the form of energy which causes the oscillations in an unstable glider. Professor Bryan and Mr. Williams have photographed gliders bearing flash-lights, and demonstrated the reality of the long and short period oscillation, but the theory needs considerable amplification so as to apply to complex cases of combined planes, and simplification so as to be readily applicable to design.

Mr. Lanchester ('Aërial Flight,' vol. ii., and 'British Association Trans.,' Dublin, 1908) gives new formulæ for the stability, and finds that the oscillations are trochoidal.*

* See 'The Engineer,' Sept. 18, 1908.

PRACTICE (AËROPLANES).

Time will not permit an exhaustive account of the theoretical principles involved to be given, but the more essential points have been touched upon, and it will be useful to indicate how these principles will be applied.

In designing an aëroplane the weight is perhaps the first consideration, and next the minimum velocity required. From formula (8) we can proceed to find S, the area.

$$W = 2\,k\,S\,V^2 \sin\gamma \cos\gamma \quad . \quad . \quad . \quad (8)$$

Let $\cos\gamma = 1$, since γ is small, and $\sin\gamma = \frac{1}{3}$, and $2\,k = \cdot 004$,* then

$$W = \cdot 0012\,S\,V^2,$$

and

$$S = \frac{W}{\cdot 0012\,V^2} \quad . \quad . \quad . \quad (15)$$

Thus, if V is 30 feet per second (say 30 miles per hour), $S = W$, i.e. the area in square feet is the same as the weight in lb. Less area will necessitate more speed, and *vice versâ.*

A useful rule connecting the area and weight (based on bird flight in spite of dimensional theory) is that

$$S \propto W^{\frac{2}{3}} \quad . \quad . \quad . \quad . \quad (16)$$

Next, to find the thrust required, we take formulæ (7) and (8), and get

$$\frac{T}{W} = \frac{R + C\,V^2}{W} = \tan\gamma + \frac{C}{2\,k\,S \sin\gamma \cos\gamma} \quad . \quad (17)$$

as the ratio between the thrust and the load. Neglecting the second term, which is small (or rather, taking a higher value for the first, so as to include the second), we write

$$\tan\gamma = \sin\gamma = \tfrac{1}{3} \text{ or } \tfrac{1}{4},$$

so that

$$T = \frac{W}{3 \text{ or } 4} \quad . \quad . \quad . \quad (18)$$

This is a very useful rule, and the author believes it was originally suggested by Professor Bryan. It is adopted by most of the French investigators.

Since the thrust per B.H.P. with a good propeller is about 30 or 40 lb., we may write

$$40\,H = \frac{W}{4},$$

* Twice ·0017 (see p. 4) plus an addition of ·0006 for the lateral spread generally employed (see p. 5).

so that

$$W = 160\ H \qquad . \qquad . \qquad . \quad (19)$$

where H is now in B.H.P.

This may be regarded as a high value, and probably only half this can be safely employed, so that 1 H.P. will carry say 80 lb. Great improvements should eventually be made in this direction.

The light motors (such as the Antoinette, Dufaux, and Esnault-Pelterie types) now made produce about 1 B.H.P. per 3 lb. of weight, or allowing for transmission gearing and friction losses, say 1 B.H.P. per 5 lb. of mechanism, so that the weight of this will be = 5 H lb., and hence from (19) (modified as suggested) we get the available weight of the surfaces, framing, and aeronaut = 75 H lb., or for framing and surfaces alone (reckoning aeronaut's weight at 150 lb.)

$$W = (75\ H - 150)\ \text{lb.} \qquad . \qquad . \qquad . \quad (20)$$

Employing the rule obtainable from (15) that S = W, we find the weight of surfaces and framing per square foot is

$$\frac{W}{S} = \frac{75}{80} - \frac{150}{S} \qquad . \qquad . \qquad . \quad (21)$$

Care must be taken to prevent surfaces interfering with one another, and this is generally attained by superposing them at a distance apart equal to their width, or placing them behind one another at the same minimum distance.

The positions of the aviator and the engines are very important. Generally the first is in front. The Wright machine has them side by side. In any case the position of the common centre of gravity must answer to the rules given in the theory of stability. Lateral balance is assured by the use of a dihedral angle between the wing planes, or by a keel plane. Captain Ferber has discovered the laws controlling the size and position of the latter, which are to be found in the paper previously referred to. Steering is accomplished in several ways, as will presently be described.

Constructive Features.

Several types of machine may be distinguished, but three especially are noteworthy, and are named after their inventors: (*a*) Chanute; (*b*) Langley; (*c*) Wright. The Phillips machine is a fourth type, but is analogous to the first. The Chanute machine is the type adopted by Farman, Delagrange, and Captain Ferber. It consists of two superposed, narrow surfaces mounted on a transverse girder. A central longitudinal girder

connects this front frame with a rear one of similar form but smaller, sometimes divided by partitions into cells after the pattern of the Hargrave kite. The aviator and motor are placed centrally at the rear of the front surfaces, where the c.g. must be, so as to be ahead of the mean centre of area of all the surfaces. The trimming planes are generally in front, and the steering planes at the rear. This differs however, and will be discussed presently. One propeller is used between the sets.

The *Langley Type*, generally termed monoplanar, consists of two pairs of wing surfaces, inclined $67\frac{1}{2}°$ from the vertical, so as to include a dihedral angle of 135°. A central shaft, or framed girder, supports the cantilever ribs which stay the wings. The engine is between the pairs of wings, and the two propellers are paired alongside.

Wright Type.—Consists simply of two superposed surfaces as in the Chanute type, with no tail. Front trimming planes similar to the main wings, and rear vertical planes for steering. Catapult initial propulsion. Two propellers behind the wings.

Phillips Machine.—Consists of a large number of superposed rigid curved-section blades, with stream line profiles, lifting by the upward diversion of the air. The frames are two or four in number, and a rear propeller is used. Torpedo-shaped body. The general principles of this machine are excellent.

Trimming and Steering.

Guide planes of various forms are used for trimming and steering. A cruciform set of planes for both purposes has been used on the Langley and Ludlow machines. Superposed pair for trimming, placed in front, have been used by Farman, Delagrange, and the Wrights. Santos Dumont (xiv. *bis*) employed a cellular kite for both purposes, and M. Bleriot has used trimming planes, turning on axes, at the tips of the wing planes. A sliding weight is used in the Weiss gliders, and the author has suggested a weight on a coarse-pitched leading screw as useful. For steering laterally, vertical surfaces are generally employed at the rear. By slightly canting the machine a lateral thrust is produced which will turn the machine, although the consequent diminution in lift tends to make it lose elevation.* The Wrights also employ torsion of the main surfaces.

Starting and Alighting.

In starting an aëroplane there are numerous difficulties. The essential is that the soaring velocity shall be reached before the machine leaves the ground. If a machine be simply

* See paper by M. Renard in 'Comptes Rendues' this year.

propelled along a track, so soon as the soaring velocity is approached the friction on the ground becomes negligible, and the propulsive effort is uncertain. Usually the machine rears or sinks forward, touches the ground, and loses its required velocity, so that no start is made. Langley experienced great difficulties in this way. Four methods are available.

1. Starting on a track which the aëroplane cannot leave until the required velocity is reached. (Langley.)

2. Starting on a track employing a small plane angle, and when a velocity has been reached in excess of the minimum for the machine, raise the planes quickly until the angle suits. The excess of speed will give the initial elevation required. (Farman, Delagrange, Ferber.)

3. Start from a height, preferably down a slope. (Voisin, Roe).

4. Use a frame which can by the store of energy in springs, or a lifted weight, act as a catapult. (Wright Brothers.)

In each case the starting device (carriage, sledge, or catapult) may be integral with, or separate from, the machine. Separately, weight is of course saved. On the other hand, the machine is useless without the hoisting device. Starting-stages with necessary catapults or other devices have been suggested. The Aëro Club de France tests machines from a steel tower in the Galerie des Machines, on the principle given third in the foregoing list.

With regard to descent, this is intimately related to gliding stability. As we have seen, if the weight is in the right place, oscillations will be damped out, and the descending machine will follow a straight descending line with a uniform velocity. The alighting springs should be capable of storing the energy of impact corresponding to this speed and angle.

Helicoptères.

It will have become evident from what has been said, that this type of machine is more or less at a discount. Machines have been made by Santos Dumont, Kress, Dufaux, and others, but as yet the results are not very important. The ability to soar is undoubtedly a great advantage, but the loss due to insufficient air supply, the absence of wedge action, and the necessity for further machinery to give lateral propulsion are great drawbacks. Mr. Rankine Kennedy is one of the strongest advocates of this type just now, and is evidently convinced as to its ultimate success. The author has interested himself in the type for a long time, but cannot say that at present he considers it to be superior to the aëroplane. In a paper just presented to the Aëronautical Society he has discussed the question.

Ornithoptères.

Professors Marey and Pettigrew have shown that the wings of flying animals rotate while reciprocating, so as to provide a forward thurst as well as a downward one. (See 'The Problem of Flight,' p. 59.) The researches of Mouillard, Langley, Fitzgerald and Deprez have also shown how the greater flying birds manage to utilise the pulsations of the wind and its vertical component to soar and glide. Lord Rayleigh has given simple rules in this connection.

A type not uncommon (on paper) is the rotating machine, in which a number of blades are controlled by a cam, so that on the downstroke they move perpendicular to their planes and on the upstroke parallel to their planes, and thus produce an upward resultant thrust The mechanical efficiency of such an arrangement cannot be so high as that of an aëroplane. Moy's aërial steamer and centrifugal fan types correspond to this variety.

Literature.

The literature of this subject is now considerable. Books and periodicals have been published in most European languages, and the Greater Powers have each military departments and scientific societies representing the movement.

Beginners in the subject would do well to commence with

- Marshall and Greenly's 'Flying Machines, Past, Present and Future' (P. Marshall & Co. 1*s.*); and
- Lord Rayleigh's 'Mechanical Principles of Flight' (Manchester Memoirs. 1*s.*).

As general books of reference the following are very useful:—

- Lanchester's 'Experiments in Aërodynamics' (Smithsonian Institution, Washingtion. 1 dollar).
- Moedebeck's 'Aëronautical Pocket Book' (Whitaker. 10*s.* 6*d.*).
- Ferber's 'Les Progrès de l'aviation par le vol plané' (in 'Revue d'Artillerie,' August, Oct. and Nov. 1905).
- Lilienthäl 'Der Vogelflug als Grundlage die Fliegekunst' (Berlin).
- Lanchester 'Aërial Flight' (2 vols. Constable. 42*s.*).

The author's books, 'The Problem of Flight,' 10*s.* 6*d.* (Griffin), and the 'The Force of the Wind,' may prove helpful.

Future Work.

Reference has been made to the necessity for further research as to the centre of pressure. Information is also wanted as to

the resistance and stability of combined planes, the thrust of screw propellers, and the effect of lateral currents on propellers and gliders. The mathematical analysis of the equations of motion of the aëroplane in space needs to be advanced. Simpler forms of the equations of stability and trajectory are required. The application of the latest investigations as to resistance (such as M. Eiffel's) and centre of pressure to these equations has yet to be made, and bird flight needs much study by ornithologists trained in applied mechanics.

Relation to War and Commerce.*

The sudden development of aërial navigation led to a popular panic which was quite baseless. At present the dirigible balloon is extremely vulnerable, cannot carry more than a few pounds weight of projectiles, and has great difficulty in hitting a mark. In espionage it may be useful. Aëroplanes may perhaps be presently available for attacking vital points and despatch work, but it will be long before they will be steady in a wind.

Commercially the outlook is worse. Although the energy required for aërial transport is not much greater than in terrestrial and marine locomotion, the danger and unpunctuality will take many years to eliminate. Wind occasionally (not frequently) will have serious effects on direction and time of passage. Eventually the airship and flying machine will affect society, but the author thinks it will not be for some years to come.

Finally, the author wishes to point out the deplorable backwardness of English invention in this direction.

DISCUSSION.

The President, in moving a vote of thanks to the author, remarked on the interest and importance of this subject at the present time. Mechanical flight was obviously one of those branches of development which depended equally upon scientific research and practical experiment; and the paper had laid open the ground for what he hoped would be a very interesting discussion. In former times, it took the missionary Moffat twelve months to travel from Cape Town to the Victoria Falls; whereas it was now possible to reach the Victoria Falls from England in about three weeks. It was claimed that mechanical flight would very shortly enable us to travel to the most distant part of the world in four days; and it was certainly a

* See article by Professor Newcomb in the 'Nineteenth Century,' Sept. 1908.

striking thing that we had reached at the present time a more advanced point in this matter than had ever been attained before.

The vote of thanks was unanimously agreed to.

Mr. Holroyd Smith, in opening the discussion, said that he had no time to test the mathematical formulæ which the paper contained. His only remark on them would be to repeat the note which the author gave at the conclusion of his paper, namely, "the mathematical analysis of the equations of motion of the aeroplane in space needs to be advanced. Simpler forms of the equations of stability and trajectory are required. The application of the latest investigations as to resistance and centre of pressure to these equations has yet to be made, and birds' flight needs much study by ornithologists trained in applied mechanics." He fully endorsed that sentence, and, as a natural deduction, was of the opinion that it was going a little too far and too fast to state definite formulæ at the present time. He remembered the statement of skilled mathematicians proving beyond contradiction that mechanical flight was impossible, and practical demonstrations had now shown their clever calculations to be wrong. In the lecture which he had given over thirty years ago, he had made bold to dispute the then statements of the mathematicians, and said, "When the navigation of the air has been actually accomplished, the mathematics will follow." The present paper showed that his prophecy had been fulfilled. His study of the question was made without the aid of any of the various books which had since been published. It was simply from careful and studious observation of the flight of birds and a natural aptitude for applied mechanics.

With regard to the author's definition of prevailing types, he thought that the aeroplane was at present the prevailing type; that the helicoptère was probable in the near future, and that the ornithoptère should be struck out altogether. He would strongly advise that neither time nor money be spent upon an attempt to make an ornithoptère, because to try to fly in the manner of birds would be just as absurd as to make a locomotive engine which would gallop like a horse.

In referring to the experiments made with helicoptères, the paper stated that there was no wedging action. If people had been trying to construct a helicoptère without realising and utilising the wedging action, they had been taking hold of the stick at the wrong end: no wonder failure followed. It must be remembered that the form of blade which would be efficient in a propeller for driving forward would be quite the reverse of efficient in a helicopteric fan. Another feature which he

wanted to mention was the question of the curved planes. The double curvature might be a very interesting part of a bird's anatomy, but he doubted its utility in mechanical flight. He was afraid that the mathematicians were indulging in a display of their knowledge of the Greek alphabet in order to find expressions for devices that would not have any useful effect. He predicted that double curved planes would never come into general use, and he based his prediction on certain experiments he made a few years ago on centrifugal pumps. He designed a centrifugal pump as nearly as he could to the best accepted mathematical formula, but as it did not answer his expectations he threw mathematical formulæ on one side, and did the work in his own way, and obtained an efficiency far in excess of anything that had previously been done. If they tried to construct a flying machine on these pretty curves they would get into a pretty mess.

In the experimental work which he carried out long ago, and described in the lecture already referred to, he discovered that, in order to get stability, they must have two sets of planes, and they must have more than the width of the planes between the two sets. He was convinced that there should be at least twice and probably three times the width of one plane between the forward plane and the rear. Most people had been trying to make the planes on a straight horizontal line. He did not think that was quite the way.

With regard to the construction of these machines, it had always been a surprise to him that there was one action of a bird which had not been utilised, and he attributed its absence to want of care in observation of how a bird steered itself. If they had carefully noticed an old crow flying across a field, they would have found that when the crow wanted to turn he twisted his tail. There was a double action in the tail of a bird—not only a lifting and depression for the up and down steering, but the sideways movement was produced, not only by a more powerful stroke with the right or left wing, as the case might be, but also by the twist of the tail, one corner being lifted when the other was lowered. This was a little but important feature which he had never seen noticed by anyone, and which could be easily and successfully applied to the steering of aëroplanes. He had given an experimental illustration of this in his lecture of over thirty years ago. At the rear of his double plane glider he had a little horizontal rudder, and by simply twisting it to right or left, and without altering the set of the planes, the glider could be made to turn in any desired direction.

Although the paper assumed to include all the points it was

necessary to consider in designing a simple type of flying machine, there was a serious omission: it was stated that the laws of hydrodynamics applied to aërodynamics. Now water was practically incompressible, and air most elastic, and its elasticity is of primary importance in the matter of aërial navigation.

It was all very well to deal with the problem of flight from the academic standpoint, but the practical phase of it was of equal importance, and in that the human element had to be considered. They might argue from the weight of a man and the strength of material, discuss the curves and the sections, the shapes of blades, and the width and length of aëroplanes, get equations and formulæ for every imaginable condition, but would that enable a man to manage a machine? As an example, take the art of skating. It would be quite easy to formulate fully on the subject, we have more certain data at our disposal, but would all the mathematics in the world enable a man who had never tried before to cut a perfect figure of eight? Just the same with billiards: one man might be able to calculate perfectly the force of the blow, the angle of incidence, etc., but never score, whilst his opponent, who knew nothing about such things, could get the balls into any pocket he liked. Take a motor car firm on its four wheels, and only the ups and downs and curves of the road to deal with, yet what motorist would trust his 40-H.P. car to the guidance of a man who had never driven one before? The human element must be considered, and if flying is to become a pastime, a machine must be constructed that can fly slowly, or how is the ordinary man to learn? In addition to pluck he must have skill and practice before he can "dare the headlong plunge through eddying gulfs of air."

The Secretary had given him permission to exhibit some of the diagrams which he made over thirty years ago to illustrate the lecture already referred to. They were painted by his own hand, when he was considering the question of flight. He would call attention to the wind table of the Smithsonian Institute, which gave useful data relative to the speed and pressure of wind when considering the matter from an engineering standpoint. The centre figure at the top represented a man who tried to fly by constructing an apparatus in imitation of a bird's wing, and the sequence was ignominious failure. Above the figure a diagram indicated the size of the wings that would be required to support a man if he had muscular energy enough to work them. The other diagrams illustrated birds in flight. One was a representation of an eagle grappling with a stork in mid-air, and showed what birds could do when they quarrelled. Another diagram illustrated a vulture, a Satanic bird, who, after

gorging on some dead carcase, had to mount heavenward again, and was therefore provided with broad and powerful wings. Starting fully loaded from the earth required wings of quite a different type from those of swallows.

It would be noticed that swallows, swifts, and sand-martins, when they started their flight, always made a downward movement first. It was that downward swoop which emphasised in his mind the necessity of a downward swoop at the commencement of mechanical flight, which gave a sudden air pressure underneath, causing a rebound that produced the required speed and buoyancy.

This brought him to another point in the paper. He could not accept the statement that the power required for increasing speed was relative to the cube of the speed. He might be mistaken, but he felt that there must be something wrong in the mathematics, for swallows might be seen any summer evening fluttering and skimming about, twisting, turning, and catching flies and travelling at not more than 6 or 7 miles an hour, and yet on their homeward journey in the autumn they travelled for hundreds of miles in one long flight at a speed of 70 or 80 miles an hour. If the power for the increase of speed was the cube of the speed, where would be the horse-power in the poor little breast of that dear little bird? It could not be. Nature was against it, and therefore let the author try to correct the mathematics on that point.

Another illustration was that of the kestrel hawk, and when he unearthed his diagrams from the dust of years, he could not help a feeling of pride at the handiwork of his younger days. The possibility of being able to hover in the air or to proceed slowly was, to his mind, the crux of the whole question, and the one which must be satisfactorily solved if aërial navigation was to become a reasonable pastime or of any commercial use. They must be able to go slowly when they wished if they were to travel safely in the air.

Major Baden Powell congratulated the author and the Society on the way in which the subject had been brought forward, for he felt that it was a matter which had been sadly neglected by British engineers. He was sure that if more engineers had taken an interest in the subject some years ago, they would now have had in England machines quite as good as those which were to be found in France and America. He hoped that the subject might be pushed on so that England might not be behindhand in the matter.

With regard to the classification of types of machines, he thought that it was rather a mistake to define the kinds of machines too clearly, and to say that they might be divided

into three classes. He could name an apparatus for flying in the air which certainly could not be included in either of the three types mentioned in the paper.

One of the first flying machines, which he had seen about 25 or 30 years ago, was such. It was a well-designed machine in its way, and was quite a type of its own. There was a propeller set at an angle of about 45°, so that it was a sort of cross between what was now called a helicoptère and an aëroplane. Then, again, they knew that Hargrave in Australia made a model which was practically an aeroplane propelled by beating wings, and it seemed to be very efficient. There were aëroplanes which were fitted with propellers to lift them directly off the ground, and which had a screw propeller behind to drive them along. So there were very often machines which did not come exactly under either of the three classifications, but were combinations of different kinds. The very word "aëroplane" was coming to be misused. Persons talked of an aëroplane as a machine of a certain type. It seemed absurd to say that So-and-so's aëroplane had two superposed aëroplanes, one in front and another behind, and so on. At present they had not got a name which exactly suited such machines. Various names had been suggested, and doubtless in time they would get one which would show exactly what was meant.

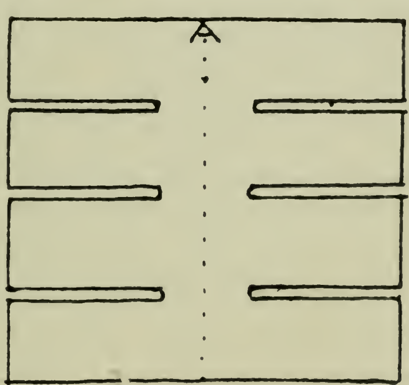
In speaking of propellers, the author had mentioned that a propeller tended to surround itself with a vortex of air. He (the speaker) was not quite sure what the author meant by that expression, but presumably he meant that there was some circular action set up in the air round the propeller. He (the speaker) had been making certain experiments with aërial propellers lately, and some careful tests as to the action of air round the propeller while it was stationary. It seemed that there was almost a calm all round the front of the propeller, whereas directly behind it there was a distinct column of air moving back in a cylindrical form. Close to that column of air were a number of little eddies, while the column itself, though it might go slightly in a spiral direction, had not at all a pronounced effect in that way. Of course, he was talking of one particular kind of propeller; others might have different effects, but the column of air moved off very nearly in a horizontal line. He had often wondered why it was that there was not a more pronounced current rushing into the opposite side of the propeller, the intake side. It was scarcely noticeable. He did not say that there was no current, but, compared with the rush of air from the off side of the propeller, it was comparatively nothing.

It would be interesting to know from what source the author got his statement that the propeller gave a thrust of from 40 to

60 pounds per H.P. He thought that such a thrust was very unusual. Many practical experiments had been made in which a thrust of from 5 lb. to 7 lb. was about as much as could be got. Mr. Sidney Hollands, who was present, claimed to have got a much greater thrust, and would, no doubt, tell them something about it, but he (Major Baden Powell) could not help thinking that anything over 20 lb. was exceptional.

He should like to ask whether the speed of the plane travelling through the air affected the shifting forward of the centre of pressure. It seemed probable that it was so, for a number of reasons, but he did not know that any actual test had been made with regard to it. Joëssel and others had made experiments with inclined planes and found that the centre of pressure shifted forward according to the angle. The less the angle the more it shifted forward, but he (Major Baden Powell) was inclined to think that when the speed of the aëroplane was increased the shifting forward of the centre of pressure was affected.

As the last speaker had said, the mathematical side of the question was one of great difficulty, and at present they were



hardly able to put the pressure on a plane with an inclined surface into a correct mathematical formula. But when it came to a curved surface, they knew very little about it. In most of the actual machines it was not merely that they had a curved surface, but they very often had complicated curves—not only fore and aft curves, but sideways curves which affected the whole question, and it seemed almost impossible to calculate mathematically what the pressure on the plane would be. Several experiments with a curved surface had proved that if the chord of the curve were absolutely horizontal, and it were placed in a current of air or driven through the air, it would

receive an upward lift, but he thought that this had never been gone into mathematically, and could not at present be explained satisfactorily.

Mr. Holroyd Smith had spoken of the stability of a plane surface. If one took a piece of paper and weighted it so as to get the centre of gravity in the right place, it would glide to a certain distance, but the least thing would upset it. It was evidently very delicately balanced. But if they split the surface up into a number of separate surfaces they would get a much better result. Nor did it depend entirely on the distance apart of the surfaces. If a sheet of paper were cut out as shown in the sketch and suitably weighted at A, it would be found to be much more stable than a plain sheet of paper of equal size, although the planes in that case were almost touching one another.

Mr. Sidney Hollands said that, as Major Baden Powell had referred to him, he felt called upon to say a word or two, but he was really not prepared to speak. In the first place, he heartily endorsed Major Baden Powell's remarks as to the extravagant claims made for thrust obtained with various types of propellers. Some years ago he made quite an exhaustive series of tests with a host of propellers of the same diameter, but of different types, forms of blade, and different degrees of cross-sectional curvature of blades and pitch. He made a series of tests with six blades, then with four blades, then with three, and finally with two, and he found the two-blade type was the most efficient. He tried with two blades of the broad-tipped types and of the narrow-tipped types, and he found that the narrow-tipped blade was the more efficient; that is to say, if the tip was one-third the width of the root and the pitch-angle was 30° at the root, and 15° at the point, giving a mean pitch-angle of $22\frac{1}{2}°$ he got the best results. With that type of propeller, 6 ft. in diameter, he got a maximum thrust of $26\frac{1}{2}$ lb. per H.P. when relatively small power was applied, and from that there was very little falling off when greater power was applied to it. In fact, he got nearly 20 lb. per H.P.

Mr. S. F. Cody said that the paper had interested him very much. He was not a trained mechanical engineer. He was a raw Western cowboy, but he was in their midst as a scientific aeronautical experimenter without any training whatever. The propeller question was a very interesting one to him. He was always searching for something that was good, and the good that he found was placed at the disposal of the British Government, which he served at the present time. He had made many experiments with propellers. He believed that the last speaker, Mr. Hollands, claimed that a propeller with a small top and large at the boss end was the best. That was the type of

propeller that he designed for the dirigible balloon which they used last year at Aldershot, but he had a very hard job to get it taken on. Nobody would believe that he was sound in putting the big end of the propeller in and the little end out. He knew nothing about the mathematical reckoning in the paper, but he had managed to build a machine from his own ideas right through, and it did fly in England, and it was the first to do so. This only showed what a raw cowboy might do if he gave his mind to it, and a raw Englishman could do the same if he had a determination to study hard.

The Antoinette engine had been referred to. That was his pet engine. It was good and light, and he had always got good work from it and had never had any trouble with it. All he had to do was to give one turn to switch on the ignition. The engine was a very good one, and he had tested propellers from it to a very great extent. He had never obtained more than 12 lb. per H.P. out of the Antoinette. He knew that with a small electro-dynamo he could get 20 or 30 lb. per H.P. with a $\frac{1}{8}$ H.P. motor. But when it came to getting a high power, somehow they lost efficiency. He did not know what the reason was. It was the same with a ship. A ship would travel 40 knots with a certain H.P., but if they had eight times the H.P. they would not get 320 knots out of the engine. He had chains to put on his machine. He had had them for a long time, but he was afraid of chains. The chains were liable to break and cause a mishap. It was said that a propeller would break. He did not know which would break first. He was afraid of chains, although he hoped to go out in ten days with chains on his machine, but he would prefer to stay near the ground. The upset that he had a little while ago was caused through going over a clump of trees, with the wind. He only got one-half of the machine over the trees, when an eddy of wind gave him an enormous lift on one side. Then the idea occurred to him to turn round, and it simply brought one of the ends downwards, and the machine tumbled to the ground. There was no fall or spill of any kind, it was merely a bad descent by striking one wing. There was no excitement at all. These machines would break when they landed, if they did not land on the right part.

He should like very much to know where he could obtain those good propellers that would give 18 lb. thrust per H.P., using 50 H.P. and not allowing the propellers to weigh more than 35 lb. Of course, if they made a propeller weighing 100 lb. it would travel slowly, and it would not do on a machine of that description. He was quite prepared to give a diameter of 10 ft., and as high as 18 lb. for the propeller. He should very much like to put the propeller of any inventor on the Govern-

ment machine and use it there, and the inventor might have it back again if he wanted it, but he would prove whether the propellers were efficient ones. He would not pay him anything for them, but he could try the War Office and see what he could get, or it would be a recommendation to him on another market. He (Mr. Cody) would make that offer. He knew it was very hard to test a propeller moving through the air, but he was inclined to try.

There was the question of curvature of the surface of aëroplanes. An aëroplane with a flat surface would be of very little use. He thought that the brothers Wright and Professor Langley had shown them almost the ideal curve for lifting.

The top part of the aëroplane or curve should be as smooth as the bottom part, or smoother. This fact he proved when he put on the propeller arm riveted to the front edge of the propeller and not the back—the inner surface, the part they were pushing with. He did not know anybody who had ever done that before. He riveted his branch upon that, and he took two propellers exactly alike and riveted the branch on the front of one and on the back of the other, or he simply took one propeller and bent it round to the same curvature the other way but all made alike, and the one with the clump on the front pulled the most. This proved that the propeller surface was exactly like the aëroplane surface.

As to the question of the bird steering himself with his tail, that might be tested on London Bridge by watching the seagulls. They had a little tail, and they did not use their tail very much, but they twisted one wing downwards or *vice versâ*. That was the way that he obtained the balance of his kite when he first started kiting: by twisting the wing of the kite. That was the method adopted by Sir Hiram Maxim, and he believed that was the greatest advantage of the propeller to-day. He had used this for fifteen years. He did not patent it, but he illustrated it in a diagram in his patent, but he did not say anything about it because he did not wish to divulge it to the country's enemies. Now, however, Wright had patented it, to prevent anybody else using it.

Mr. Sidney H. Hollands said that he might be allowed to remark that he designed his propeller over twelve years ago and obtained his results at that time. He might further remark that he was quite prepared to supply a propeller that would do what Mr. Cody required. As to the conditions stated by that gentleman, he thought that they needed to be revised.

Mr. S. F. Cody said that he might answer that by saying that he designed his propeller up at the Crystal Palace first before he went to the War Office. That was about three years ago. He

had no knowledge of anybody else ever designing the propeller before. The idea struck him as a practical one some twelve years ago, when he was a cowboy. He was not a mechanical engineer at all, and he did not copy Mr. Hollands' propeller, though Mr. Hollands may have been first with this type of blade.

Col. Fullerton, R.E. said that the first point he wished to draw attention to was the question of air friction. Professor Zahm and other experimenters stated that the friction of the air was very considerable, while Sir H. Maxim and Professor Langley considered that it hardly existed. Such a difference of opinion was somewhat difficult to account for, but it seemed possible that the large amount of friction found by Professor Zahm was due more to the head resistance of the bodies experimented with, than the actual force exerted by the air on their sides. The bodies were *not really fair shaped*, and in such cases it was very difficult to distinguish between the real air friction, and the aërodynamic resistance of the bodies. Further experiments on air friction were much required.

Next as regarded the "centre of pressure." It seemed to him that this was rather a complicated question, because the term was used to mean two quite different points, viz. the point where the line of action of the air force cut the surface under consideration, and also the point where the resultant of all the air forces acting on the surfaces met. The latter was the correct definition, as if the surface had an appreciable thickness, it was difficult to say exactly where the line of action of the air force struck it. He mentioned that he had made a number of experiments concerning the position of the centre of pressure some years ago, and found that, with certain kinds of curved surfaces, the centre of pressure always remained in the same place, for all angles of inclination between $+15°$ and $-15°$. With bodies, the results were very similar, but there was a slight change of position as regards the vertical plane perpendicular to the longer axis of the bodies, and, in designing such forms, it would be necessary to allow for this.

Then as regards the ratio of lift to thrust, he thought the author was rather pessimistic, in putting it at 3 or 4 to 1. Twenty years ago Mr. Horatio Phillips and Sir H. Maxim got 5 to 1, the Messrs. Wright now get about 6 to 1, and Mr. Weiss appeared to do better still. He did not want to suggest too high a figure, but he thought that a machine in which the ratio of lift to thrust was less than 5 to 1, was hardly worth considering. As regards the thrust of propellers, he must say that he was rather staggered at the estimate of 40 lb. per B.H.P. He believed that Sir H. Maxim and Mr. H. Phillips had got about 7 lb. or so, and that the Messrs. Wright had got nearly 16 lb.,

and this latter amount appeared to be as much as could be safely allowed for. Assuming this view to be correct, the weight carried per B.H.P. would be about 80 lb., which was more than had yet been accomplished.

As regards longitudinal stability, he considered that the experiments he had lately made, showed that automatic stability was apparently quite feasible. The balance of a machine really depended on the *good design* of its different parts, and the provision of complicated regulating planes, gyroscopes, etc., was quite unnecessary.

Mr. E. V. Hammond said that they had heard statements from several speakers as to the high thrust per H.P. which had been obtained by various types of screw propeller.

The experimenters stated that they had used some 4 or 5 H.P. to rotate the screw, and had obtained thrusts of about 25 lb. per H.P. This might be correct for the H.P. employed, but it was known that the H.P. absorbed by a plane surface passing through the air increased as the cube of its velocity, and therefore these high thrusts would fall to about 14 or 15 lb. per H.P., when the same propeller was coupled to a 50 H.P. engine.

Most of these tests had been carried out with the propeller stationary, but when an aëroplane was in full flight, the air flowed off the back of the propeller at the speed at which the machine was travelling, plus the displacement of the propeller itself. For this reason, the efficiency of a propeller under working conditions, fell still more; and would probably result in a total thrust of some 10 lb. per H.P.

Mr. Howard T. Wright said that he had listened with very great pleasure to Mr. Chatley's valuable paper. He felt sure that if every one about to construct an aëroplane would study the paper, and act on its suggestions, he would be saved a great deal of disappointment. About 50 per cent. of the machines which had been brought to him recently had not the slightest chance of flying, because they were based upon principles contrary to natural laws, and not a few persons tried to introduce their old friend "perpetual motion" into the aëroplane, although they would probably repudiate the suggestion that they intended to do so. He quite agreed with the author that further research was necessary, although he might differ from him as to what direction it should take. If the clubs which were devoted to this branch of engineering would devote their funds to encouraging research rather than to social or even practical considerations, the matter would be further advanced. He thought that research would be best carried out by the amalgamation of two gentlemen, one a scientific mathematician, and the other a purely practical man who had

had some experience of flying machines. Many of the formulæ were quite valueless because they had been carried out by scientific men, and, though they might be perfectly accurate, they were of very little use.

There was a great deal of confusion on the question of propellers. He was particularly pleased with the first part of the author's remarks. If everyone would thoroughly understand the marine propeller before commencing to design an air propeller, and would learn the difference between an air fan and an air propeller, better results would be got. Doubtless, there was confusion in measuring the thrust of a fan, owing to the fact that it was generally measured when the fan was stationary. In Wright's machine they were told that there was an efficiency of 70 per cent. In the Farman machine there was probably 50 per cent., and in an Esnault Pelterie machine that he had seen at work, there was an efficiency of 80 per cent. when the machine was travelling at its correct speed. If a propeller was designed for one speed, it was right for that speed. Aëro-propellers could probably be designed to give a slip of 15 per cent., and with that there should be no difficulty in getting 80 per cent. efficiency. In the machine that he was building there were quite enough points to consider without bothering about propellers, so he simply went to a company and said, "Give me one of your 80 per cent. efficient propellers." And they did this for 20*l.*, and guaranteed it.

In reference to stability, it would, of course, be interesting to know exactly where the horizontal centre of pressure occurred, but he was sure that it was possible to design a machine without knowing exactly where this point was.

The author had made no reference to vertical centre of pressure, and it would be interesting to know whether he had any formula for it. Of course, it would be very complicated, because one had to take into consideration wind resistance and various other points. The propeller, of course, must be placed on the vertical centre of pressure. They had, no doubt, noticed the great difference in the relative positions of the propellers in Farman's and Wright's machines. In Wright's machine it was almost exactly half-way between the two planes, and in Farman's machine it was very near the lower plane.

A point of extreme importance to which the author had not referred was the question of mechanical strength. He thought that quite 75 per cent. of the failures in aëroplane construction were owing to their lack of mechanical strength. Some machines, if they did not really break, became, at least, so deformed, that the beautifully calculated curves were worse than useless.

There was another extremely important point for beginners learning to fly. One had to learn to fly just as one had to learn to ride a bicycle or to skate. The difficulty was that, as most machines were constructed, it was impossible to learn how to fly until one got into the air. In all the machines that he was constructing it was possible to learn vertical and horizontal steering and also lateral stability, and not only to learn them on the ground, but to learn them separately so that, when the aviator got into the air, he knew how to steer in every direction. This was a most important point.

As to the question of starting, he could not understand the author's remarks where he said, "If a machine be simply propelled along a track, so soon as the soaring velocity is approached the friction on the ground becomes negligible, and the propulsive effort is uncertain." Reference was evidently made here to propelling machines along the ground by means of their own wheels. If this was so, surely as soon as the resistance of the road wheels became negligible the propulsion was nil, and not uncertain. At any rate, he had not heard of any serious attempt to propel an aëroplane by means of road wheels. The propeller must always be relied upon for giving the initial velocity. Wright simply used the catapult for getting speed more quickly. This was proved by the fact that when Wright won the prize for high flight he did not use a catapult, because one of the conditions of the prize was that the winner should use no outside help at starting. He got over the difficulty by simply increasing the length of his starting rails. The author was a little wrong in the third form of starting: "Start from a height, preferably down a slope," and he mentioned Voisin and Roe. He (Mr. Wright) did not know that Roe's machine had yet risen by its own power. But it was impossible to fly at all until one had sufficient propeller thrust to drive the machine not only along the level ground at its soaring velocity, but also slightly uphill, because, as soon as the machine left the ground, it really ran uphill. Unless there was sufficient thrust to get over that, it was impossible to fly.

He should like to give a description of an extremely interesting machine which he had built. It was a machine which did not come into either of the three categories mentioned by the author. It was, roughly, a large plane in the shape of a triangle—a single plane having 600 square feet of area. The base of the triangle was the leading edge, and, at each of the two extreme corners, there was a propeller 20 feet in diameter, running at a speed of 120 revolutions per minute. 10 H.P. net was applied at each fan spindle, and he got a direct lift of 325 lb. for each fan, or 650 lb. on the whole machine. This was

rather better than the formula given by the author, but it was only fair to say that the propellers were very peculiar. They were so arranged that on the outward stroke they had an angle of incidence of 42°, and on the inward part of the stroke they only had an angle of 12°, so that the power was really intermittent. The inventor's idea was that the machine should be raised into the air, and that when it got to a certain height, the speed of the propeller would be reduced, and the machine would glide forward at its own plane, the centre of gravity being well in front. The first machine weighed 1200 lb. The lifting effect was only 650 lb. Another machine of this type, which would be tested shortly, had a total weight of 600 lb., including the driver. Personally, he did not believe in light motors, although in this case a motor was used with a weight of 120 lb. for 30 B.H.P. The fans were 26 feet in diameter, running at 75 revolutions a minute.

In conclusion, he would say that, if there was anyone present who thought of building an aëroplane, he must not have the idea that he could find some new invention that would solve the problem in a few minutes. It was only by patient thought and great consideration of detail that it was to be done. He had himself devoted two years entirely to the consideration of the manner of constructing main planes, and the result was that he had got an effect 15 per cent. better than anyone else. And, whatever else an inventor might do, he should not stand at Westminster Bridge and watch the birds. He would only obstruct the traffic, and he would not get very much further in that way. Someone had come to him the other day, and said that he had been to Westminster Bridge, and had invented an aëroplane that could do everything that a bird could do except peck and eat. He (Mr. Wright) expressed his interest, and the gentleman promised to let him know when his model was completed. This was six months ago, but he had not heard anything more about the matter.

Mr. F. J. Hargreaves said that he wished to ask why the author thought that it would be a long time before the aëroplane would be steady in the wind.

Mr. P. L. Senecal complimented Mr. Chatley on his paper, and said that he did not rely entirely upon mathematical formulæ. Navier's formula asserted that it required a force of 30 H.P. to enable a duck to fly, and was accepted at the time as a scientific fact, but was now known as "l'erreur de Navier." This formula had retarded aeronautical work for over a century. It had been proved over and over again, that as experiment progressed theory progressed with it. It was experiment coupled with perseverance and hard work that had accomplished all that had been done up

to the present in the science of aeronautics. Experimenters in aviation persevered until at last they evolved the glider. They then learned to guide, balance and control their gliders in a satisfactory manner. Then and not till then did they add the motor. The result of this dogged perseverance was seen in the machines of the brothers Wright, Messrs. Farman, Delagrange, Bleriot and others. Nearly 40 years ago he became one of the original members of the Aeronautical Society of Great Britain, and at that time he made a model of a helicoptère which was a modification of the flying bats now to be seen in the toy shops. When wound up it ascended screwing itself round the room and describing a low-grade spiral till it reached the ceiling, where it rebounded, dropping five or six feet and ascending again. This happened five times in succession. By this time the motor having run out the screw stopped working, and the machine laid itself on the air, so to speak, and glided downward round the room in the same manner in which it had ascended. He mentioned this simply to show that the helicoptère in that form could be used, he believed, to rise from the ground, to travel at an incline and to travel horizontally as well. It would be a very simple machine, if they could raise it from the ground, just to clear the grass, or glide at pleasure by merely stopping the motor.

He had made many aëroplanes in his time in various forms. He had made mechanical flies and birds, and other things to fly, but he had come to the conclusion that, owing to the difficulty of keeping the centre of gravity coincident with the centre of pressure—that was, of preserving stability—it was almost impossible to keep the aeroplane steady at all velocities and in all pressures of the wind. He had, therefore, designed a helicoptère that would have the centre of gravity in the centre of the machine, and a circular pressure around that centre with a shifting weight to alter the centre of gravity if required. The effect of this was that, as soon as he shifted the weight forward, the helicoptère would travel in the direction of the weight and the wind. If he moved it further, it would travel faster until the weight overcame the pressure of the air under the screws (which, of course, should never be allowed), but by shifting the weight backwards or forwards as required, he could modify the speed of the machine. The small, compact and stable helicoptère would eventually take the place of the airship and aëroplane, because of the unwieldy bulk of the airship and the large dimensions and inactive surfaces of the aëroplane, and also because of the practical impossibility of maintaining equilibrium in all kinds of weather. He did not say that the helicoptère would take the form he had just mentioned. There was room for improvement, but here was the foundation of a

practicable, reliable and stable aërial machine. Two screws working in opposite directions would shift the weight, and thereby change the direction of motion, and, by modifying the distance of the weight from the centre of the machine, they could travel at any speed from 1 mile up to 60 miles an hour, or more.

He would pass now to the theory of flying. He found that it was not necessary to have an even surface, but the under surface should be rough ; it should be, in fact, feathers or down or velvet, or something of that kind, to prevent the air from escaping too quickly from under the wings, so as to obtain maximum support. The upper part of the wings should be perfectly smooth to let the air pass over as quickly as possible and offer the least resistance upward.

Mr. Holroyd Smith had spoken of a bird's tail twisting and being useful for steering. He did not believe in the tail much. It was evident that pigeons and sparrows were very frequently without any tails. He did not know that they could not steer themselves much more quickly without. Here was another fallacy about the theory of the speed of flies. Some years ago, it was asserted that a fly, in flying, moved its wings 300 to 500 times a second. He made a very careful experiment, and found, by the length of the wings, that it was impossible for a fly to move its wings at such a rate. In the cold spring weather, they might often see bluebottles flying; they could just move their wings, but could not fly very fast. By watching their wings, one could count the number of vibrations, and as it is known that the eye can only detect 13 or 14 vibrations per second, it is evident that the number of strokes must be below that number, and he had sometimes found it as low as 8 strokes per second. When the fly was alarmed it would travel faster, and in the warm sunshiny weather one could not see the strokes, because the fly moved its wings too rapidly.

He had destroyed the natural balance of a number of flies by stopping the vibrations of the balancers, and had then re-established that balance artificially. He believed that he was the only person who had made the experiment, but he might say that anything acting as a drag behind the fly effectually restored the artificial balance in the act of flying.

This was an interesting subject. He found that the vibration of the wings of a fly was the same as the action of two screw propellers working in opposite directions.

He hoped that the paper and the discussion which had followed it might bring practical and theoretical men together. If this discussion had taken place forty years ago, when the Aeronautical Society was founded, they would not now have to

deplore that England was ten years behind other countries on these subjects.

Mr. G. L. O. Davidson said that he was afraid that what he had to say might be somewhat in the nature of a bomb. He had been studying mechanical flight for nearly twenty-five years, so that he might be considered as knowing just a little about it. Twenty-five years ago people who believed in mechanical flight were considered lunatics, but now all this had been altered, and it was gratifying to find the subject being discussed by the Society of Engineers. It was, he thought, a subject which ought to be approached by engineers. Mr. Chatley had gone into most wonderful calculations, and other gentlemen had done the same, and yet, until Mr. Senecal got up, he did not hear a single word as to the fundamental principle of flight. Fourteen years ago he had asked Sir Hiram Maxim at the Society of Arts whether he could tell them "how a bird flew," and Sir Hiram at once jumped to the conclusion that the speaker wanted to make a machine to fly like a bird, with flapping wings, and said he had no doubt that he could make a locomotive to gallop along the ground. They did not, however, want to do that. If they were going into mechanics they must generally have a rotary motion to do the same work as the reciprocal action of nature. In travelling along the ground they must have a wheel to do the same work as the legs of an animal; and if they were going through water they must have a rotary equivalent to the reciprocal action of the tail of a fish. They had this in the screw propeller. Could anyone truthfully say that a bird twisted itself through the air with its tail? and yet that is what all aëroplanes are trying to do. They had, in fact, done so, and it was very clever of them, but that was not flying. Propelling a structure through air as a vessel was propelled through the water was not flight as he understood it, that is to say as a bird flew.

Before they began to make their calculations they must find out what was the fundamental principle of flight. As a matter of fact, it was simplicity itself. He could explain the theory of flight in two minutes. How did a bird progress through the air? It would be admitted that a bird weighed something, and it was heavier than the air.

Let them take a bird or machine, preferably the "gyropter," which has lifted $1\frac{1}{2}$ tons with 40 H.P. or 75 lb. per H.P. He would suppose that the bird or the machine was either on the ground or a foot or a mile off the ground. He would suppose that the bird or the machine weighed 1 unit (1 oz. or 1 ton). They could also throw in that the bird or the machine had some momentum, or that it was at rest. Under all these circumstances the bird or the machine must have an

absolutely vertical downward pull of gravity equal to 1, its weight. He would suppose that by means of the reciprocal action of the wings of the bird, or by the rotary action which must be employed in mechanics, they exerted a thrust in an absolutely vertical upward direction equal to 1. Then the bird or the machine would remain where it was so long as the upward pull was equal to 1. Now he would suppose that the pull of 1 in the upward direction was the millionth part of a degree out of the vertical. Would the bird or the machine then remain where it was? It could not. It would go off in the direction of exactly half the angle created by the upward and downward pull. He would then suppose that the pull in the upward direction was a little bit more than 1. The resultant would naturally go off above half the angle, and if the upward pull was sufficiently in excess of 1, the body would move forwards and upwards; and so long as one could keep the upward pull sufficiently in excess of 1 and keep it in advance of the vertical, the speed would go on increasing until it was counteracted by the resistance to the air presented by the cutting edge of the machine, which would probably not occur at a less speed than 200 or 300 miles an hour. What it resolved itself into was to be able to get an upward pull sufficiently in excess of the downward pull of gravity, and to be able to govern the angle of that upward pull. In America he had constructed a rotary wing flying machine or "gyropter" as he called it. The two rotary wings or gyropters, each 27½ ft. in diameter, consisted of a series of aëroplanes or aërocurves, 110 to each wing, and by forcing these aërocurves through the air at a speed of 55 revolutions per minute, he had obtained, with a single gyropter, an absolute lift of 1½ tons with less than 40 H.P., or a total of 3 tons with 80 H.P., using both gyropters. He proposed building another machine in this country, as, unfortunately, he had had a little accident through want of money to obtain suitable engines for the original experimental machine. He thought that it was important that they should look to the fundamental principle of flight, and then they could make their calculations. It was not good to make calculations without basing those calculations upon the fundamental principle. He hoped that the Society of Engineers would take up this matter in a serious way.

Mr. Holroyd Smith said that he was sorry to interrupt, but he should at once dispute that the speaker's diagram had anything to do with the fundamental principle of a bird's flight. It might be, and probably was, a correct explanation of the speaker's machine, but it was absolutely different from the manner in which any bird ever did or ever would fly.

Mr. R. W. A. Brewer wrote saying, that although the information in the paper covered a good deal of theoretical ground,

he thought that practical experiments would show fallacies in many of the assumptions which formed the first principles of the formulæ given. No hard-and-fast rules could yet be stated as to the weights which could be sustained in the air when a plane or curve was propelled by an engine of any given horse-power.

Theoretical data could only be usefully based upon the result of practical experiment—and the author gave no practical information to substantiate any of his theoretical deductions.

Mr. Henry Farman attributed his success not so much to any form or shape of the apparatus employed, as to his own methodical tests and modifications which were made as the results of his experiments. He had also found that *weight* was not so important as resistance to penetration, and that it was preferable to make a part of greater weight if by this means less resistance to air could be secured.

Increased length of flight had been attained by reducing air friction, this having been made possible when portions of irregular shape had been covered with canvas.

The author did not make sufficient point of the fact that the rear shape of a body suspended in an air current was of far more importance than the nose or front portion. Eddies depended so much upon the closing of the parted air *after* the body had passed.

With reference to propellers at high speeds, flimsy blades were much distorted, and this accounted for the great loss of efficiency in many cases.

The position and number of the propellers was not alluded to by the author in his paper, and it would be interesting to know the effect of a single propeller upon lateral stability as compared with the balancing effect of two—and what the difference of stability would be in a lateral direction between the double-propeller machine of the Wright Bros. and Farman's single-propeller machine.

The author had given a short outline of the arrangement of several machines, but the paper would have been of much more practical utility had he stated the effects of the various differences in several notable types.

For instance, what were the advantages of a biplane over a single plane, and of leading elevating planes over rear planes?

On pages 248 and 249, the subject of centre of pressure was considered, and the diagrams would be of greater utility had the location of such points been given for several values of γ. In the second condition of longitudinal stability given on page 249, the position of the centre of gravity was discussed with reference to that of the centre of area of sustaining planes, but in no case did the author locate this latter position in his diagrams.

The writer had done this for Fig. 5 by taking moments of the surfaces about the rear extremity of the machine, and he found that the centre of areas calculated in this way fell just in front of the propeller, one third of the distance between it and the centre of gravity of the whole machine.

A detailed discussion of the various features of *one* particular type of machine together with calculations based on the formulæ given in the paper would have been of great practical utility, and would have shown the necessity for any modifications which in actual practice had been found to increase the efficiency of that machine.

Mr. Hollands also wrote referring to the remarks of Messrs. H. Wright and Hammond, to which he had no opportunity of replying at the meeting, that he had had the privilege of being associated with Sir Hiram Maxim in his experiments and trials at Bexley (1892–4), and was enabled to say that—contrary to the unproved statements of Messrs. Wright and Hammond—one result of Sir Hiram's very carefully conducted tests, was to prove that *the thrust is practically the same whether the propeller was standing or advancing* (there was no error about this), and his own experimental experience was the same.

Moreover, Mr. Curtiss in America had found this to be the case.

Sir Hiram Maxim had published the above result.

Mr. Albert P. Thurston wrote, saying that since it was more economical to give a large mass of air a small velocity, than to give a small mass a high velocity, it followed that a screw or helicoptère should be designed to engage as much air as possible. The author had stated that the action of an air propeller was almost identical with that of a marine propeller except for the difference of density, which was in the ratio of about 1 : 800, the resistance being in the same ratio, and that the coefficient of skin friction was about 5 per cent. of the coefficient of resistance. It should not be forgotten, however, that water wetted a surface, whereas air did not; water possessed viscosity and cohesion, and air had no measurable viscosity, and its molecules were always tending to get as far away from one another as possible. Clerk Maxwell had stated that the skin friction would be, not 5 per cent., but $\frac{1}{50}$ of 1 per cent. Sir Hiram Maxim, Professor Langley and the Wright Brothers all stated that the skin friction was exceedingly small. The resistance to motion of an aeroplane was due not so much to skin friction as to eddies set up by parts of the body which were not of stream line formation.

If the vicinity of a propeller was explored by means of short ribbons, it would be found that the air entered the propeller

from the front and sides and went out from the back in a straight column. Sir Hiram Maxim had found that there was no centrifugal or fan-blower action until the pitch angle exceeded 45°.

With stationary propellers suitable for driving aëroplanes,

$$\text{Horse-power} = \frac{\text{thrust} \times \text{stationary slip}}{33{,}000}$$

With such propellers, the thrust did not fall off very greatly up to a speed of 70 per cent. of the stationary slip or theoretical speed.

It would be seen from this that the thrust was proportional to the speed, and that the power was proportional to the *square* of the speed. The author, however, had stated that the power varied as the *cube* of the speed. This statement would apply only to helicoptères of large diameter and to aëroplanes of fixed inclination.

Since, with an aëroplane, the inclination could be diminished with an increase of speed, the horse-power required to drive an aëroplane, under ordinary conditions, would be less than the cube of the speed.

With regard to the author's statement that the thrust was proportional to the area of the blades, a two-bladed propeller was found to be the most efficient form. With practical speeds and diameters the addition of an extra pair of blades gave no additional thrust, and hence all flying machines had only two blades.

With regard to the thrust of a propeller, this depended on the speed with which it travelled axially. If the screw were perfectly efficient, a thrust of 40 lb. per H.P. could not be obtained at a greater speed than 9·4 miles per hour.

The thrust given by equation (18) appeared to be unduly high. The gliding angle of existing machines was apparently between 1 in 5 and 1 in 8. Equation (18) could be safely written

$$T = \frac{W}{6}$$

The less the theoretical speed or slip of a propeller, and the greater the horse-power to be transmitted, the greater should be the diameter.

Sir George Cayley had shown in 1795 that if a weightless screw, 200 feet in diameter could be constructed, it would enable a man to raise himself from the ground by his own power. Sir Hiram Maxim had recently stated that if the diameter of the screw were raised to 2000 feet, the power of a man would

sustain the weight of a horse, while if the diameter were reduced to 20 feet, then it would need the power of a horse to sustain the weight of a man.

The efficiency of the Wright machine as compared with the Farman machine was partly due to the fact that the propellers of the former engaged nearly 7 times more air per horse-power than the propeller of the latter.

With increased screw diameters, helicoptères became more efficient, not only because more air was engaged, but also because still air was struck, and before the air was fully set in motion the aëroplane had passed on.

The aëroplane was more efficient than a helicoptère could be, because it represented the case of a helicoptère plane at an infinite radius. Also, when a helicoptère was given a lateral motion, it was subjected to a considerable cross torque due to the velocity being increased on one side, and decreased on the other, with respect to the air. The aeroplane had no such disadvantages. It could be constructed so as to engage the maximum of air with the minimum of resistance and weight, and by placing the propellers behind the centre of maximum resistance, some of the energy lost by the disturbance of the air could be recovered by the propellers.

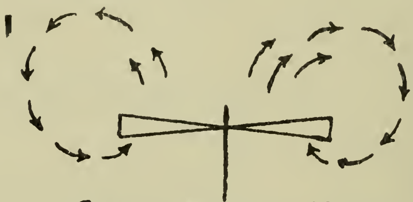
With regard to the change of the centre of pressure, a paper by Dr. Stanton, (Proc. Inst.C.E., vol. clvi.), was of interest. He found that the change of the centre of pressure with the inclination of an aëroplane was brought about largely by the change of distribution of the suction on the back of the plane.

Mr. Herbert Chatley, in replying after the discussion, referred to the following matters :—

Classification of Types.—Major Baden Powell and Mr. Wright had suggested that there were certain types of machine which could not be brought under the headings. The author perfectly agreed that there were intermediate types, and had no intention of making the classification a very rigid one. Some kind of system must, however, be used in describing the machines. He had suggested a scheme for classifying the different species of aëroplane in a paper which was shortly to appear in the Journal of the Royal Society of Arts.

Propeller Action.—The vortex about a propeller with 100 per cent. slip, criticised by Major Baden Powell, was observed by Professor Thurston (see correspondence in 'Engineering' this year), and the author had found a somewhat similar motion in a small stationary propeller placed vertically in a fluid. The fluid was ejected circumferentially and axially, so that the general flow was outward, and the fluid descended outside the field of action of the propeller, and returned to the same near

the tip circle (see sketch). With regard to the thrust, the figures quoted were based on the claims of Kress, Vogt, Hammond, Pickering and other experimenters. The author thought that three points were commonly overlooked in considering this question.



SECTION THROUGH VORTEX.

(1) That the thrust neither needed to be, nor could be, great when the slip was small.

(2) That the propeller settled down to a speed determined by the characteristics of the *motor*, and that the torque, and consequently the thrust, would vary in a manner depending on the motor as well as on the actual form of the propeller.

(3) That a propeller suitable at one speed might be useless at another.

Lift to Drift Ratio.—Col. Fullerton had expressed the hope that the author's value (3 or 4) for this, would soon be exceeded. This probably would be so, but at the same time it must be realised that the direct resistance of the framing would always reduce this ratio to considerably less than the lift to drift ratio of the aërofoils themselves. Phillips had aërocurves with a ratio of 20 to 1, but the thrust required was considerably more than $\frac{1}{20}$ of the weight.

Friction.—In regard to this question, the author ventured to contest Lanchester's and Zahm's results to a certain extent. Mr. Lanchester included in his skin friction coefficient, the dynamic action of the edge, which would appear more properly to belong to the "aërodynamic resistance" of the aërofoils. Nevertheless, the author had convinced himself from Turnbull's experiments, that the friction had generally been under-estimated. Professor Zahm's results did not seem to allow for the increase in viscosity due to the proximity of the walls of the testing tunnel.

Mathematical Theory.—The author had been criticised by Mr. Holroyd Smith and Mr. Senecal on account of the theoretical matter introduced, and the latter had stated that an adherence to theory had led Navier to severely check the progess of aviation in the nineteenth century. The author pointed out,

however, that Navier's theory was simply based on Newton's hypothesis of fluid resistance, whereas every rule and formula given in the paper was based *on experiment and nothing else*, with the sole exception of the theory of stability. It was not sufficiently realised by the "naturalist" aviator that mathematics was infallible if the underlying assumptions were true. Now the assumptions in this case were those derived (not as assumptions but as conclusions), from the experiments of Wenham, Langley, Eiffel and others, who certainly could not be convinced of unpracticality.

With regard to the theory of stability, Professor Bryan, its chief exponent in this country, had acknowledged that it was almost valueless until certain experiments had been made. In writing to the author, however, on the subject of this paper, he said that the completion of this theory (with the necessary experimental data) was essential to the progress of aviation.

In further support of the use of mathematics, it seemed scarcely necessary to mention that the present development of the steam turbine and reciprocating engines and also of electrical machinery was almost wholly due to the results of successful "x-chasing."

Horse-Power.—With regard to the variation in power with speed, by reason of the possible reduction of the gliding angle, it did not necessarily follow that the variation would follow the cubic law. This was explained in the paper. The actual index of variation might be anything from 1 to 3, and would increase with the speed by reason of the friction.

Centre of Pressure.—Major Baden Powell's query as to the variation of the centre of pressure with the velocity was very pertinent. Since the angle of attack need not be so great at high speeds, the centre of pressure would tend to advance. An examination of the experimental results of Kummer, Langley and Turnbull did not, however, indicate any variation in the position of the centre of pressure due to variation of velocity *per se*.

In reply to Mr. Brewer's communication, the author thought that this gentleman had made the same error as some of the other critics of the paper, viz.:—neglect of well-known experimental data. Mr. Brewer asked for confirmation of the formulæ. In regard to formulæ 1, 2, and 3, the records of Dines, Froude, Langley, Stanton, Vince, Thibault, Rayleigh and many another could be quoted. In regard to formula 5, the references in the footnote would afford ample justification. Rules 6, 7, 8, and 9 were simply deduced from the preceding and the triangle of forces. For 11 and 12 reference was given. Nos. 13 and 14 were given by Captain Ferber who first assisted M. Voisin, the maker of the Farman and Delagrange machines.

The rules in the section of "Practice" were deduced from the preceding, with an ample allowance for experimental error.

With regard to the rule connecting weight with power, the author would point out that No. 18 had been given by Captain Ferber in the discussion which followed the author's paper before the Junior Institution of Engineers in February 1908, and that No. 19 as modified in the text only involved a thrust per H.P. of 20 lb.

The "reduction of the air friction," the author presumed, referred to the reduction of dynamic resistance. Friction was a matter of surface, not shape.

The "rear form" was mentioned in the paper, and, after the important results obtained by Mr. Froude and his son in connection with ships, scarcely needed emphasizing.

Flimsy blades were undoubtedly to some extent the cause of inefficiency, but probably the principal cause was bad shape for the particular speeds. The author would point out here that the product of the thrusts (in lb.) per H.P. into the speed of the aëroplane could not exceed 550 foot-lb. per second, so that at speeds of 27 feet per second, 20 lb. per H.P. thrust was the maximum.

With regard to the stability effect of propellers, undoubtedly the gyrostatic action of one propeller was objectionable, but since the propeller was generally of small mass, and the turning speed (not the rotation of the propeller) was also small, the torque on the aëroplane due to this effect would not be great.

The relative advantages of different systems of planes had not been experimentally settled, but it would seem that they principally affected the facility of construction rather than the action.

Mr. Brewer's calculation as to the centre of areas was interesting, but not quite to the point, as the diagram was only intended to represent the arrangement of the machine. However, he showed that the centre of gravity is in front of the centre of areas, which is the great essential.

The suggestion as to calculations based on one machine had already been carried out by Col. Fullerton in reference to general machines, the results having been given in the 'Aëronautical Journal.' There was always the difficulty in this case of obtaining the requisite data.

The author would again repeat that the calculations were based on experiments, and were of such a simple character that actual disproof of the assumptions or the working would be necessary before they could be said to be inapplicable.

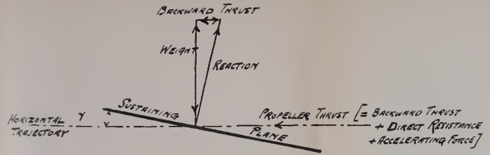


FIG. 1.—EQUILIBRIUM OF FORCES IN AËROPLANES. AËROPLANE RUNNING HORIZONTALLY.

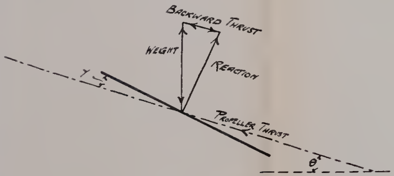


FIG. 2.—EQUILIBRIUM OF FORCES IN AËROPLANES. AËROPLANE ASCENDING.

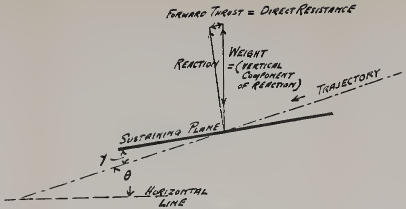


FIG. 3.—EQUILIBRIUM OF FORCES IN AËROPLANES. AËROPLANE DESCENDING (GLIDING).

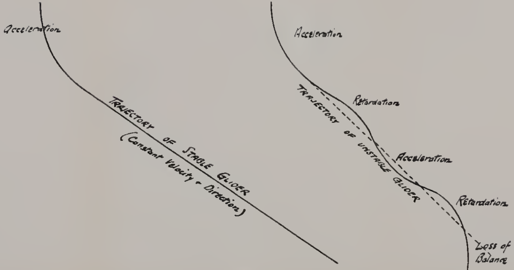


FIG. 4.—TRAJECTORIES OF STABLE AND UNSTABLE AËROPLANES.

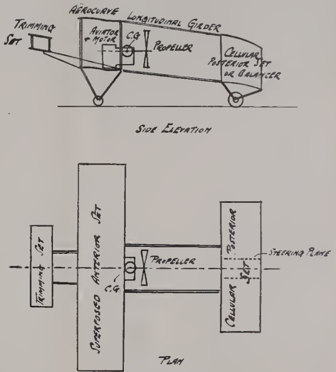


FIG. 5.—CHANUTE TYPE OF MACHINE.

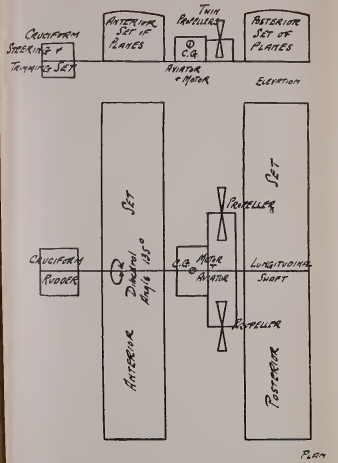


Fig. 6.—Langley Type of Machine.

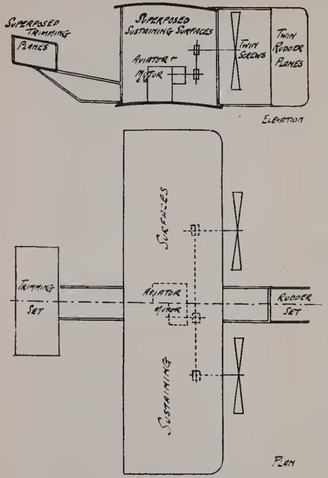


Fig. 7.—Wright Type of Machine.

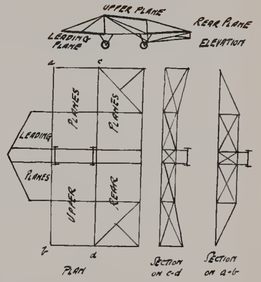
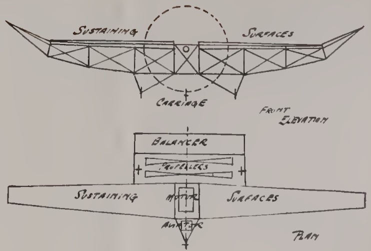


Fig. 8.

Aëroplane
designed by
Lieut. J. W. Dunne
& Professor
Huntington
[Pat No 2808-1908]



Aëroplane
designed by
Mr F. W. Lanchester
[Pat. No 9143, 1907]

Fig. 9

RECENT ENGLISH MACHINES.

Obituary.

William Thomson, Baron Kelvin of Largs, O.M., P.C., G.C.V.O., D.C.L. (Oxon.), LL.D. (Cantab.), whose death occurred on December 17, 1907, was born at Belfast on June 26, 1824, and after a course of study at the University of Glasgow, entered St. Peter's College, Cambridge, in 1841, was placed Second Wrangler and first Smith's prizeman in 1845, and was subsequently elected a Fellow of his college. In 1846, at the age of 22, he was appointed Professor of Natural Philosophy at the University of Glasgow, a position which he held for fifty-three years, and in 1904 he was elected Chancellor of the University with which he had been so long associated.

He became a Fellow of the Royal Society in 1851, and occupied the Presidential chair in 1890. A knighthood was conferred upon him in 1866 for services rendered in connection with laying the Atlantic cable, and in 1892 he was elevated to the peerage.

Having regard to the remarkable series of mathematical and physical investigations carried out by him, Lord Kelvin may be regarded as the foremost physicist and most accomplished scientist of his time. He combined the highest mathematical ability and great originality with the power of applying his vast attainments to practical and industrial problems, and as an engineer he contributed largely to the sciences of navigation, telegraphy, and electrical engineering. He was a tireless worker, taking the keenest interest in the advancement of science to the very end of his life. His remains were interred in Westminster Abbey, in a grave adjoining that of Newton. Lord Kelvin was an Honorary Member of the Society from 1890 until his decease.

Sir George Barclay Bruce, Honorary Member, who died on August 25, 1908, after a long illness, was one of the very few men of the present time who have not only seen the development of the railway systems of the world, but who were actually alive when railways came into existence.

He was born on October 1, 1821, and was the son of Mr. John Bruce, of Newcastle-on-Tyne, a schoolmaster, at whose school he attended until he was 15 years of age, and in this connection it is interesting to note that Robert Stephenson and

Sir Isaac Lowthian Bell (another deceased Honorary Member of this Society), were also educated there. Upon leaving his father's school, young Bruce entered the Stephensons' works, where he served a six years' apprenticeship, afterwards taking service with the firm as an engineer.

His whole life was identified with railway construction, and the railways with which he was connected in a greater or less degree included lines not only in England and India, but also in South America, Germany and Spain.

Mr. Bruce was President of the Institution of Civil Engineers in 1887–88, and in the latter year he received the honour of knighthood.

Sir George had been an Honorary Member of the Society of Engineers since 1891.

Charles Rous-Marten, who died on April 20, 1908, was well known as an authority on locomotive engineering in general, and train speeds in particular.

His "Report to the New Zealand Government on British Express Speeds"—the outcome of months of patient investigation—first brought him into notice on this side of the globe, but he had been busy for some years before with the improvement of the New Zealand State Railways, especially in regard to locomotive practice. He was the first to call attention to the wonderful work done on French Railways by the 4-cylinder de Glehn du Bousquet compounds and in some special experiments arranged at his request, he obtained up-hill speeds with big loads which are still world's records for that class of work. But Mr. Rous-Marten's activities were not confined to locomotive work; his first scientific achievements were in meteorology in New Zealand, where he set up, and for ten years carried on, the most southern weather observatory in the world, at a point near Foveaux Strait. He was a good linguist, a skilled musical critic, and took an active part in public life in New Zealand. He was elected a Member of the Society in 1901, and was the author of two papers on locomotive work, for one of which ("Notes on English and French Compound Locomotives") he was awarded a Society's Premium of books.

FIFTY-FOURTH ANNUAL GENERAL MEETING.

HELD AT
THE OFFICES OF THE SOCIETY, 17 VICTORIA STREET,
WESTMINSTER.

Monday, December 14, 1908.

JOSEPH WILLIAM WILSON, President,
in the Chair.

The Minutes of the Fifty-Third Annual General Meeting, held December 9, 1907, were read, confirmed, and signed.

The President read the report submitted by the scrutineers of the balloting papers for the election of the council and officers for the year 1909, and announced the names of the elected members.

The President then announced the premiums which had been awarded by the Council for meritorious papers read during the year. (See the Report of the Council, page 286.)

A vote of thanks was accorded to the scrutineers, and the proceedings terminated with a vote of thanks to the President, Council and Officers for their services during the year 1908, and was duly acknowledged.

ANNUAL REPORT OF THE COUNCIL, 1908.

In presenting their report for 1908, the Council have pleasure in stating that the fifty-fourth year of the Society's work has been one of steady progress. The losses from deaths, resignations, and other causes, reach the unusually large aggregate of 46, due partly to the fact that the Council deemed it advisable to strike off the roll of the Society a number of members whose subscriptions were considerably in arrears, in preference to allowing the arrears to accumulate still further, but these losses have been more than counterbalanced by the election of 49 new members and associates during the year, and the total membership at December 31, 1908, constitutes a record for the Society, being one in excess of the previous highest total of 550 at the end of 1904. The following comparative table indicates the losses and gains in membership during the last five years.

—	Years.				
	1904.*	1905.	1906.	1907.	1908.
Losses due to deaths, resignations and other causes	30	46	37	37	46
Gains due to elections of new members and associates	40	39	40	39	49

* Previous record total membership in this year.

The Membership Roll.

The total numbers of Hon. Members, Members and Associates at the ends of the years 1906, 1907, and 1908, were those given below.

Class.	Dec. 31, 1906.	Dec. 31, 1907.	Dec. 31, 1908.
Honorary Members	19	20	20
Members	376	376	360
Associates	151	152	171
Totals	546	548	551

The Council regret to state that seven deaths have been reported to them during the year, including that of Sir George Barclay Bruce, Hon. Member, and of Mr. Thos. Andrews, F.R.S., who was elected as a member in 1870, and had been awarded three premiums in respect of papers read before the Society.

Finance.

The Balance Sheet and Revenue and Expenditure Account appear on pages 290 and 291. The Balance Sheet shows a small excess of expenditure over income amounting to 6*l.* 7*s.* 1*d.* This, however, is to be compared with the corresponding loss of 60*l.* 17*s.* 7*d.* for the year 1906, when the previous conversazione was held. Under "Liabilities," the item "Sundry Creditors" is high because of three accounts, namely, Messrs. Spons', the rent of offices, and the Secretary's salary, which in previous years have been settled just before the end of the year. On the Assets side of the Balance Sheet a good feature is that the subscriptions in arrear amount to only 194*l.* 18*s.* 6*d.* as compared with 249*l.* 4*s.* 6*d.* in 1907. The cash at the bank shows a large increase, being 332*l.* 0*s.* 10*d.* as compared with 166*l.* 4*s.* 4*d.* in 1907, and 90*l.* 1*s.* 5*d.* in 1906. It must be noted, however, that "Sundry Creditors" being about 100*l.* higher this year, the cash at the bank may be taken to be roughly 232*l.* The increase in the office furniture account is due to the purchase of a typewriter, but a 10 per cent. depreciation has again been written off.

Referring to the Income and Expenditure Account, the cost of the Transactions appears somewhat higher than in 1907, though very little higher than in 1906. The amount for 1908 is high owing to the new method of charging for the work, rendered possible by printing the Transactions in two forms, namely in yearly volumes as heretofore, and in eight part form. Consequently the sum of 225*l.* 3*s.* includes both the cost of the Transactions for 1907, and nearly the whole of the cost of those for 1908.

On the income side, the admission fees show an increase of about 7*l.* and the interest on deposit an increase of 2*l.* 2*s.*

Amalgamation with the Civil and Mechanical Engineers' Society.

The question of amalgamation of this Society with the Civil and Mechanical Engineers' Society, has been advanced to a definite arrangement. A meeting of the joint committee of the Society of Engineers and the Civil and Mechanical Engineers' Society, was held at 17 Victoria Street, Westminster, on March 10, 1908, when details of a provisional agreement were arrived at, this agreement being afterwards confirmed at Council meetings of the respective societies. A revision of the rules is in progress, and when this work is completed the new rules will be submitted to the members of the two societies for approval. Subject to this approval being obtained, the amalga-

mation will probably be effected in May 1909, at the close of the Jubilee Session of the Civil and Mechanical Engineers' Society. The Council are confident that the union of the two Societies will preserve and improve the standing and usefulness of both.

Meetings.

The Council have held 11 meetings, and there have been 17 meetings of the various committees. Three visits to works of engineering interest took place during the vacation, and eight ordinary meetings have been held at the Royal United Service Institution.

The papers read during 1908 have maintained a high standard of excellence, while the discussions have been of value in further elucidating the subjects of the papers, and the Council hope that during 1909 the members and associates will show a still greater appreciation of these meetings, and take a more active interest in the work of the Society by attending in large numbers, and contributing to the discussions.

Papers.

The following is the list of papers read during the past year :—

- Feb. 3.—President's Inaugural Address, by Mr. J. W. Wilson.
- Mar. 2.—The Treatment and Formation of Road Surfaces, by Mr. A. J. Metcalfe.
- April 6.—The Destruction of Arch Bridges, by Mr. H. C. Duncan Scott.
- May 4.—The Design and the Waste and Wear of Wheel Teeth, by Professor R. H. Smith.
- June 1.—The Engineering Pros and Cons of the Metric System, by Mr. A. H. Allen.
- Oct. 6.—The History of Mechanical Traction on Tramways and Roads, by Mr. H. Conradi.
- Nov. 2.—The Flow of Liquid Fuel through Carburettor Nozzles, by Mr. R. W. A. Brewer.
- Dec. 7.—Mechanical Flight, by Mr. H. Chatley.

Premiums.

The Council have awarded Premiums to the following authors in respect of papers read by them and given in the above list.

1. To Prof. R. H. Smith, the President's Gold Medal.
2. To Mr. Herbert Chatley, the "Bessemer Premium" of Books.
3. To Mr. A. H. Allen, a "Society's Premium" of Books.
4. To Mr. H. Conradi, a "Society's Premium" of Books.
5. To Mr. H. C. D. Scott, a "Society's Premium" of Books.

Visits to Works.

The following works were visited during the vacation, and particulars thereof appear in the Transactions (page 157):—

June 27, 1908.—The National Physical Laboratory.
July 14, 1908.—The Whitefriars Glass Works.
Sept. 22, 1908.—The Admiralty Harbour, Dover.

Annual General Meeting.

The fifty-fourth Annual General Meeting was held at the Society's offices on December 14, 1908, and a report of the proceedings appears in the Transactions (page 283).

Annual Dinner.

The fifty-fourth Annual Dinner was held at the Waldorf Hotel on Wednesday, December 16, 1908, Mr. J. W. Wilson presiding. The visitors included Admiral the Hon. Sir Edmund R. Fremantle, G.C.B.; Lieut.-Gen. D. Hutchinson, C.S.I.; Sir Alexander Binnie, Past-President Inst. C.E.; Mr. W. M. Mordey, M. Inst. C.E., President Inst. Electrical Engineers; Mr. W. Noble Twelvetrees, M.I.M.E., A.M.I.C.E., President Civil and Mechanical Engineers' Society; Mr. H. D. Searles-Wood, F.R.I.B.A., Chairman Royal Sanitary Inst.; Rev. Theodore Wood, F.E.S.; and Mr. E. Schenk, Chairman of the Crystal Palace Company. The music was under the direction of Mr. Murray Rumsey.

Exchange Transactions.

The Society continues the exchange of Transactions with the following Institutions, the volumes being available for reference by members and associates at the Society's offices.

The Institution of Civil Engineers.
The Institution of Mechanical Engineers.
The Institution of Electrical Engineers.
The Institute of Naval Architects.
The Iron and Steel Institute.
The Surveyors' Institution.
The Association of Municipal and County Engineers.
The Civil and Mechanical Engineers' Society.
The Junior Institution of Engineers.
The Institution of Mining and Metallurgy.
The Royal Engineers' Institute.
The Incorporated Institution of Gas Engineers.
The Royal Institute of British Architects.
The Chartered Institute of Patent Agents.
The Royal Society of Arts.
The Liverpool Engineering Society.

The Cleveland Institution of Engineers.
The North East Coast Institution of Engineers and Shipbuilders.
The North of England Institute of Mining and Mechanical Engineers.
The South Wales Institute of Mining Engineers.
The Institution of Engineers and Shipbuilders in Scotland.
The Institution of Civil Engineers of Ireland.
The French Institution of Civil Engineers.
The Canadian Civil Engineers' Society.
The Victorian Institute of Engineers.
The Engineering Association of New South Wales.
The American Society of Civil Engineers.
The Municipal Engineers of New York.
The Association of Engineering Societies.
The Smithsonian Institution.
The Franklin Institute.

Journals and Magazines.

The library contains many books on engineering subjects, and, in addition, the following journals and magazines are supplied gratuitously by their respective proprietors, all being available for reference by the members.

American Machinist.
Arms and Explosives.
Automotor Journal.
British Architect.
Builder.
Building News.
Cassier's Magazine.
Commercial Motor.
Concrete.
Contract Journal.
Electrical Review.
Electricity.
Engineer.
Engineering.
Engineering Magazine.
Gas and Oil Engine Record.
Illuminating Engineer.
Indian Engineering.
Indian and Eastern Engineer.
Journal of Gas Lighting.
Machinery.
Machinery Market.
Marine Engineer.
Mechanical Engineer.
Mechanical World.
Motor Traction.
Page's Weekly.
Power.
Royal Society of Arts Journal.
Sanitary Record.
Scientific Monthly.
Shipping World.
South African Engineering.
Steamship.
Surveyor.
The Metal Industry.
The Quarry.
The Tramway and Railway World.
Water.

Practical Assistance to Members.

The Council desire to call special attention to several schemes recently initiated for the benefit of the members and associates. Notice of these matters has been sent to all members and associates, but in some cases it may not have been realised that the following arrangements are actually in force :—

1. Members and associates may obtain free legal and general advice with regard to professional matters (only). Statements of questions on which advice is required should be made in writing to the Secretary, and it is hoped that this new undertaking will be of equal benefit to both town, country, and foreign members.

2. The *Business Directory*, containing particulars of the leading classes of work undertaken by members, has already proved useful on several occasions when inquiries have been received for the names of engineers with experience in particular classes of work. The Directory, however, is by no means complete, and all who have not yet sent in their 5-inch by 3-inch card-index cards are urged to do so without delay.

3. The *Employment Register* for the use of engineering assistants has been utilised by a number of members and associates, but this might be rendered of much more advantage if all members requiring assistants would apply to the Society and make the register known to employers of engineers. No charge is made is connection with this work, and the Council hope that it will receive continued and increasing support.

4. In the new rules now being drafted, provision will be made to enable members, under certain conditions, to bring questions affecting the interests or work of the Society before all the members, and to obtain their votes thereon by post without the necessity of calling a special general meeting.

The Council have also discussed the important matter of Registration, and other questions affecting the status of engineers generally.

Practical suggestions from members for improving the status of the engineering profession, and detailed information regarding fees, salaries, and duties of engineers and engineering officials, in this country or abroad, will be of great assistance to the Council in this connection.

Finally, the Council hope to receive the active assistance of all the members, whether in London, the provinces, or abroad, in carrying on the work of the Society, for substantial progress can only be secured with the cordial co-operation of each individual member.

January, 1909.

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED DECEMBER 31, 1908.

Dr.

Year ended 31 Dec., '07. £ s. d.	Expenditure.		£ s. d.
185 19 7	To Printing Transactions	£252 7 5	
13 0 6	*Less* Sales	27 4 5	225 3 0
172 19 1			
122 16 0	,, Rent of Offices and Hire of Rooms		110 4 0
231 0 0	,, Salary of Secretary and Clerical Assistance		210 0 0
45 17 7	,, Stationery and General Printing		49 14 5
15 4 6	,, Reporting Meetings..		17 4 0
6 5 4	,, Refreshments for Members at Ordinary Meetings ..		6 7 7
20 1 0	,, Fuel, Lighting, Office Cleaning and Repairs		17 17 8
42 4 7	,, Postage, Telegrams, Carriage, and General Expenses		58 6 5
11 17 10	,, Dinner—Balance		19 6 11
– – –	,, Conversazione		60 10 1
5 16 3	,, Law Charges		0 10 6
4 16 6	,, Depreciation on Office Furniture		5 16 10
25 0 0	,, Donation to Mrs. Nursey, Widow of late Secretary..		– – –
98 7 9	,, Balance carried to Accumulated Fund Account—Excess of Income over Expenditure for the year ..		– – –
£302 6 5			£781 1 5

Cr.

Year ended 31 Dec., '07. £ s. d.	Income.		£ s. d.
	By Subscriptions for the Year	763 7 0	
	Less amounts written off	71 18 6	
728 3 6			691 8 6
6 12 0	,, Life Subscriptions—proportion for 1908		5 18 10
49 7 0	,, Admission Fees (including £11 11*s.* 0*d.* unpaid at date)		56 3 6
1 13 0	,, Interest on Deposit		3 18 2
16 10 11	,, Interest on Investment (L. & N. W. Rly. Debenture Stock)		17 5 4
– – –	,, Balance carried to Accumulated Fund Account—Excess of Expenditure over Income for the year ..		6 7 1
£802 6 5			£781 1 5

LIFE MEMBERSHIP FUND.

£ s. d.		£ s. d.
6 12 0	To Revenue Account—proportion for the year	5 18 10
59 8 5	,, Balance to be carried to next Account..	53 9 7
£66 0 5		£59 8 5

£ s. d.		£ s. d.
66 0 5	By Balance brought from last Account, being unexhausted Balance of the Fund on 31st December, 1907	59 8 5
£66 0 5		£59 8 5

PREMIUM FUND.

£ s. d.		£ s. d.
15 18 9	To Premiums	15 9 0
32 1 7	,, Balance to be carried to next Account	32 8 1
£48 0 4		£47 17 1

£ s. d.		£ s. d.
32 4 10	By Balance brought forward from 31st December, 1907..	32 1 7
10 10 0	,, President's Donation	10 10 0
5 5 6	,, Interest to date, Bessemer Trust Fund Investment ..	5 5 6
£48 0 4		£47 17 1

NOTE.—£185 North Eastern Railway Stock is held in trust to secure payment of the late Sir H. Bessemer's premium.

BALANCE SHEET, DECEMBER 31, 1908.

Year ended 31st Dec. '07. £ s. d.	LIABILITIES.		£ s. d.
25 0 9	Sundry Creditors		144 19 11
59 8 5	Life Membership Fund		55 9 7
32 1 7	Premiums Fund		32 8 1
25 0 0	Special Fund		20 0 0
25 6 6	"Nursey Memorial Fund"		46 4 5
25 0 0	J. Bernay's Legacy		25 0 0
34 2 6	Subscriptions Received in Advance		25 7 0
	Accumulated Fund:—		
	Balance at 31st December, 1907	£859 16 7	
859 16 7	*Deduct* Excess of Expenditure over Income for the year ended 31st December, 1908 ..	6 7 1	853 9 6
£1,085 16 4			£1,200 18 6

Year ended 31st Dec. '07. £ s. d.	ASSETS.		£ s. d.
240 6 0	Subscriptions in Arrear	£183 7 6	
8 18 6	Admission Fees in Arrear	11 11 0	
249 4 6		194 18 6	
100 0 0	*Less* Reserve against Bad Debts	100 0 0	
149 4 6			94 18 6
166 4 4	Cash at Bank		
	Current Account ..	17 0 10	
	Deposit Account ..	315 0 0	
			332 0 10
25 0 0	Special Account		20 0 0
5 0 11	,, in hands of Secretary		5 17 6
606 0 0	Investment—£606 London & North Western Railway Company 3 per cent. Debenture Stock		606 0 0
43 8 6	Office Furniture		52 11 8
40 0 0	Library		40 0 0
38 19 6	Stock of Transactions		38 19 0
11 18 7	Sundry Debtors		10 11 0
£1,085 16 4			£1,200 18 6

Audited and found correct,

(Signed) SAMUEL WOOD, F.C.A.,
Hon. Auditor.

13th *February*, 1909.

PREMIUMS FOR 1908.

At an Ordinary Meeting of the Society, held on February 1st, 1909, the following Premiums were presented, viz. :—

The President's Gold Medal to :

Prof. R. H. Smith, for his paper on The Design and the Waste and Wear of Wheel Teeth.

The Bessemer Premium of Books to :

Herbert Chatley, for his paper on Mechanical Flight.

A Society's Premium of Books to :

A. H. Allen, for his paper on The Engineering Pros and Cons of the Metric System.

A Society's Premium of Books to :

H. Conradi, for his paper on The History of Mechanical Traction on Tramways and Roads.

A Society's Premium of Books to :

H. C. Duncan Scott, for his paper on The Destruction of Arch Bridges.

SOCIETY OF ENGINEERS.

INDEX TO TRANSACTIONS

1857 to 1908.

AUTHORS.

SUBJECTS.

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